Chronica Botanica

Agronomia — Biologia — Genetica

Chronica Botanica

An International Collection of Studies in THE METHOD and HISTORY of BIOLOGY and AGRICULTURE

edited by FRANS VERDOORN, PH. D.

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Planten, Boomen ende Blommen
Will' ick singhen, will' ick rommen,
Ende prysen door mijn dicht
Dat als heden comt in't licht.
Maer wat will' ick hulpe vraeghen
Van u-lieden? wat behaeghen
Vind' ick in een saeck versiert?
Beter word van my gheviert
Dorothé die maeght ghepresen,
Dese sal my jonstigh wesen:
Dese zy myn Sang-goddin'
Eer ick myn ghedicht beghin'.
Weest dan myne Patronesse,
Weest dan myn' Advocatesse
Dorothé, thoont my uw' jonst
En versterckt myn swacke const.
Eerst sing'ick de Lauwerieren
Want sy plachten t'hooft te cieren
In des keysers Julî tijdt,
Vande winners in den strijdt.
Mijrthus, Daedels, Oleander,
Citroen, Oraignen en meer ander
Cypres en Granaden schoon,
Die verdienen schier de croon
Van de boomen al-te-saemen:
Voort noch hondert ander naemen
Als Amandels, Roosmarijn,
Abricocken, Barbarijn.
Vyghe-boomen, en Syringhen
En Juniperus wy singhen:
En de moeder vanden wijn
Die magh niet vergheten zijn.
Nu will' ick de Blommen prijsen,
En die lof en eer bewysen
Soo om hunnen soeten geur,
Als wel om het schoon coleur.
Hyachinten, Tulipanten,
Violetten, Amaranten
Iris, Croon imperiael,
Violettjens Matronael.
Animonien, Narcissen
Angelieren, schoone Klissen
Van de Roosen wit en root,
En meer Blommen kleyn en groot,
Aquileyen, Martagonen,
Fraxinellen en Pionen,
Schoon Ranonckels, Sonne-blom
Goudt-blom, dat is willekom
Schier by alderhande menschen
(Want naer t'goudt zijn jders wensche)
Maer ick buyten doel hier schiet',
'tGoudt en is de Blomme niet.
Daer ick eerst quam af te spreken:
'tGoudt dan sal ick laeten steken,
Al-hoe-wel het noodigh is
Totter Blommen coop ghewis.
Ick moet' oock de Leli' loven
Want sy gaet het al te boven
Midts sy suyverheydt beelt-uyt,
En die in sich-self besluyt.
Crocus, Honts-tandt, en de Blommen
Die wy naer de Lenten noemen,
En die voeren 'slevers naem
Oock bekent door haere faem,
Beiren-ooren, Colchicaenen
Elleboor', Ameriaenen
En meer Blommen welckers naem
Tot myn dicht zijn onbequaem.
Dus sal ick een eynde maken,
En myn corte dichtjens staken.
Want ick niet bequaem en ben
Om te prysen door myn pen
All' de Blommen in't besonder,
En voorwaer dat is gheen wonder,
Want al quam Homerus weêr,
Hy en soude nimmermeer
All de Boomen ende Blommen
Naer verdiensten connen rommen.

H. I. B. Reyntkens (1676)

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The ROYAL BOTANICAL EXPEDITION to NEW SPAIN

THE CITY OF MEXICO IN 1823, FROM TACUBAYA. — On the left is the hill of Chapultepec, crowned by the unfinished Royal Palace. Notice the "terrenos pantanosos" between Chapultepec and the city; at the farther margin of this area the first site was selected for the Botanical Garden. All this area is now part of the city. (*From* BULLOCK).

The ROYAL BOTANICAL EXPEDITION *to* NEW SPAIN

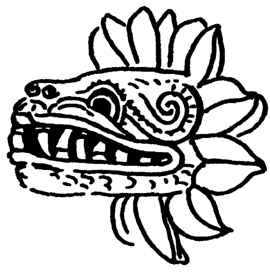
1788-1820

as described in documents in the
Archivo General de la Nación
[Mexico]

now translated and collated

by

HARALD WILLIAM RICKETT
Bibliographer, The New York Botanical Garden



H. W. Rickett *was born 30 July, 1896, near Birmingham, England. Educated in St. Paul's School, Kensington, until 1911; then in Pawling School, New York, Harvard University, and the University of Wisconsin. Second Lieutenant of Infantry in the American Expeditionary Force, 1917 to 1918. Successively assistant, instructor, assistant professor, and associate professor of botany, first in the University of Wisconsin, from 1924 to 1939 in the University of Missouri. Bibliographer, New York Botanical Garden, since 1939. Editor of the Torrey Botanical Club, since 1940.*

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INTRODUCTORY NOTE

The national archives of Mexico, *Archivo General de la Nación,* are housed in the Palacio Nacional on the Plaza Mayor. They are bound in some thousands of volumes, forming several series. In the series entitled *Historia,* volumes 460-466 contain the records of the *Expedición Botánica* sent by CHARLES III in 1787 to survey the "natural productions" of his domain of Nueva España and to establish there a botanical garden. These papers I was privileged to sample during a short stay in Mexico in 1943 as a grantee of the Committee for Inter-American Intellectual and Artistic Cooperation. From them, with a few other contemporary papers, I have attempted in the following pages to reconstruct the history of botany in Mexico from 1787 to 1820. I am indebted to Dr. JIMÉNEZ RUEDA, Director of the *Archivo General,* for the courtesy extended to me in permitting consultation of these files.

In the colonial administration of the end of the eighteenth century, any matter of business, for instance a request for repairs in the Botanical Garden, was passed from officer to officer, from Viceroy to Attorney General to Assessor to Architect and back again to Viceroy, with endorsements, requests for surveys and estimates, with receipts and affidavits, with notations of all kinds, forming an *expediente,* or, as we should say, a file. From the statements of expenses incurred, which are generally itemized, we can obtain an accurate picture of the operation of the Botanical Garden and of the itinerary of the Expedition. Since each *expediente* included copies of all relevant correspondence, we can gather a fairly complete idea of the personalities involved and their problems.

Many documents have obviously been lost; but most of the files are surprisingly complete. They are bound in the volumes with a generally faulty attempt at chronological sequence; some papers are evidently displaced from one file to another. The files are numbered (or sometimes misnumbered) consecutively in each volume, and the sheets in each file numbered, sometimes on both sides, more usually on one; in volume 460 the sheets are numbered consecutively throughout. My problem was obviously one of collation. Even if all sheets were placed in chronological order, many events would be found cropping out in various connections and at different times. When numerous desultory references were brought together a complete picture often emerged.

In order to facilitate further reference by anyone interested, I have endeavored to give chapter and verse, as it were, for all statements made. In the citations *Archivo General de la Nación, Historia* is abbreviated AGH; after this is placed the number of the volume, followed by a colon; the *expediente* is designated by a Roman numeral, which is followed by the number of the sheet or sheets.

Chapter I

SESSÉ: ESTABLISHMENT OF THE ROYAL BOTANICAL GARDEN

"Our Catholic Monarch, promoting in his vast American dominions a scientific Expedition to collect, identify, and methodically describe the natural productions of the three Kingdoms of Nature, decided at the same time to create a Botanical Garden in this Capital, on the same lines as that of Madrid, where the fruits of the Expedition might be preserved, with a Chair of Botany, in which his beloved subjects of New Spain might be instructed in this important Science."[1]

On the first day of May, in the year 1788, at five in the afternoon, the study of botany was formally inaugurated in the Hall of Dissertations (*General de Actos*) of the Royal and Pontifical University of Mexico. The ceremony began with an elegant and instructive discourse delivered with great energy by the Director of the Botanical Expedition and Garden, Don MARTIN DE SESSÉ.[2] Having pointed out the antiquity of botany, the high esteem in which it was held by the great men of all nations, and its usefulness to the state, he went on to expound its relations with commerce, agriculture, economy, and other arts, and particularly with medicine, finally urging young men to dedicate their lives to the study of natural history in all its three branches.

The occasion was dignified by the attendance of the Royal Audience,[3] of professors, of clerics of all denominations, of noblemen, soldiers, and others of high rank. SESSÉ and CERVANTES were installed as professors before the Rector of the University. In the absence of the Viceroy, official Protector of the Garden, the Regent of the Royal Audience, Don FRANCISCO XAVIER GAMBOA, presided.[4]

At seven in the evening, the balconies of the University were illuminated and the Director provided an orchestra which discoursed harmoniously in the Hall of the Faculty (*Sala de Claustros*) to divert the crowd gathered to see the fireworks. These had been arranged by the skillful Master of Pyrotechnics Don JOAQUÍN GAVILÁN. Three trees, of the kind known in Mexico as papaya, imitating nature in leaves, flowers, and fruits, represented sex in plants; the sexes being separate in this species. Two female trees bore flowers and fruits of various sizes; a male tree stood between them and emitted from its flowers sparks of fire towards the females, perfectly representing the transfer of pollen through the air. At the foot of the male tree were various devices alluding to the growth of a garden, which illuminated the square with ingenious and brilliant lights of many colors;

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[1] Gaz. Mex. 3: 75 (supl.). 6 My 1788. The following three paragraphs are a free and slightly condensed translation of p. 75-77 of the same issue.

[2] According to COLMEIRO (Bot. Hisp.-Lusit. 11), published in Mexico as a pamphlet, and in Madrid in Mem. Lit. It was advertised for sale in Gaz. Mex. 3: 104 (supl.). 24 Je 1788.

[3] *Real Audiencia*, a sort of Supreme Court.

[4] AGH 461: II 1. In a letter dated 30 April 1788 the Viceroy, MANUEL ANTONIO FLOREZ, expresses his inability to attend and asks the Regent to preside.

and others no less entertaining. As the three trees faded from sight, in place of the male appeared an inscription in letters of fire which read AMOR URIT PLANTAS; a quotation from the ingenious work of LINNAEUS entitled *Sponsalia plantarum*.

His Majesty CHARLES III, King of Spain, will be remembered, long after interest has waned in his wars and politics, for his encouragement of the scientific exploration of his wide dominions. In 1786 MUTIS was busy with the flora of Nueva Granada (Colombia and Ecuador). RUIZ and PAVON were engaged upon their celebrated survey of the plants of Peru and Chile. NÉE, HAENKE, and PINEDA were shortly to start on their voyage around the world with MALASPINA, during which they collected plants on the coasts of California and in other parts of the Americas. Nueva España, one of the most important and fertile of the Spanish possessions, alone lacked a scientific expedition.

Various persons are credited with the original proposal to establish a botanical garden in Mexico. From letters in the *Archivo General*, it would seem that that honor belongs to its first Director, Don MARTIN DE SESSÉ Y LACASTA. In an official controversy over the date from which salaries should be paid, reference is made to 3 August 1785 as the date of his "first representation" on this matter.[5] In one of his own letters he gives some account of his early career and claims to have suggested the idea to His Majesty.[6] He was a physician who had served with the army under Don MARTIN DE ALVAREZ Y SOTOMAYOR in the famous siege of Gibraltar of 1779-1783, and had later gone to the Americas under Don VICTORIO DE NAVIA. When he was in Havana he had advanced money to the Royal Treasury—10,000 pesos; this, he said, was what had prevented him from returning to Spain, for the loan was never repaid. Instead he went to Mexico, where he acted as physician to the Holy Office and in the hospital named of the Love of God. For two years he assisted in the prisons and houses of correction. Then, "moved by humanity, by the honor of the nation and the good of the state, he proposed to Your Majesty that you would be pleased to establish in this capital a Royal Garden with a Chair of Botany, and to examine the very fertile provinces of these vast domains. . . ."

Whatever the source of inspiration, there resulted a Royal Order of 27 October 1786, to establish a botanical garden and a scientific expedition "to make drawings, collect the natural products, and illustrate and complete the work of Doctor Don FRANCISCO HERNÁNDEZ." A second order, dated 13 March 1787, clothed these generalities with particulars, naming the scientific members of the expedition and prescribing their emoluments, privileges, and duties. A third, dated 23 November 1787, gave directions for the operation of the garden, including a plan of instruction in botany.[7] SESSÉ was named Director, at a salary of 2,000 pesos a year, which was to be doubled for such periods as he was in the field on duty. Other members of what came to be known as the Botanical Expedition were Don JAYME SENSEVE, "profesor farmacéutico," Don JUAN DE CASTILLO (then in Puerto

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[5] Letter of 11 November 1788. AGH 462: I 15, 16.

[6] Petition to His Majesty, 27 June 1792. AGH 460: 238-243.

[7] AGH 462: I 2; V 37. A copy of the Regulations of the Garden and Plan of Instruction in Botany is in AGH 466: VIII 20-33.

THE PRESENT PARK OF CHAPULTEPEC.—The hill ("cerro") in the background is crowned by MAXIMILIAN'S palace, now a museum. Here stood the old Royal Palace in the days of the Expedition, and here SESSÉ proposed to establish the Botanical Garden.

THE VILLAGE OF SUCHILAPAN on the Isthmus of Tehuantepec. Such villages afforded MOCIÑO and LA CERDA lodging in their journey to Guatemala in 1796.

Rico[8]), and Don JOSEF LONGINOS MARTÍNEZ, "naturalista," (which meant about what "biologist" does now to the lay public—one interested in the study of animal life). These were to receive 1,000 pesos a year, also doubled for time spent in the field. JUAN DE DIOS VICENTE DE LA CERDA and ATANASIO ECHEVERRÍA were the artists, at 500 pesos (AGH 462: I 7). Finally VICENTE CERVANTES was appointed "catedrático" or professor, to take charge of instruction in the new Garden.

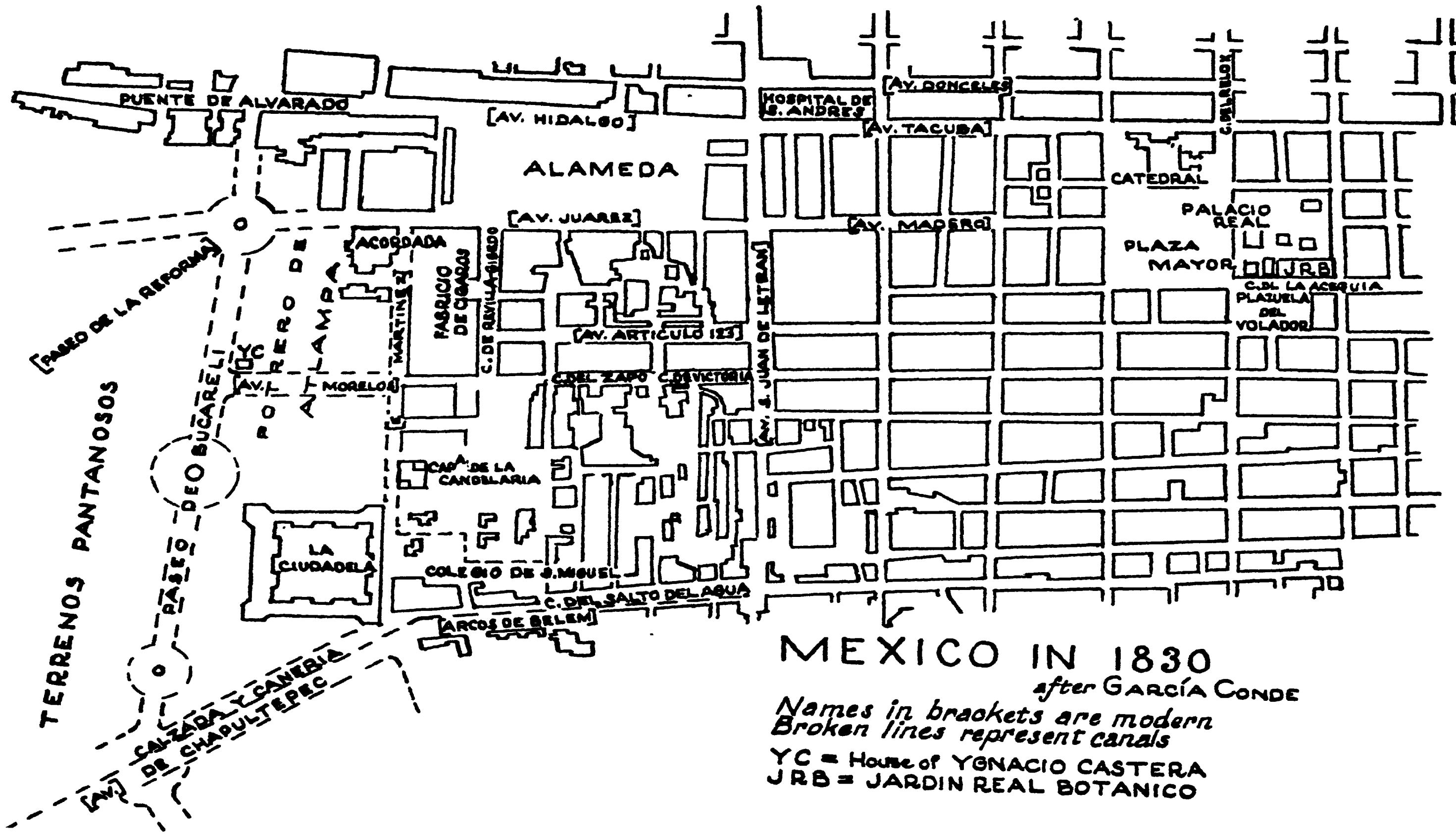
VICENTE CERVANTES was born in 1755 in Safra, Badajoz, in the old province of Estremadura.[9] He early demonstrated an aptitude for study, and inclined towards pharmacy; but his family was too poor to pay for entrance into the University. VICENTE came to Madrid and became an apprenticed clerk (*depedniente y discípulo*) in a drugshop. In his leisure time he studied botany, getting the lessons at second hand from a friend who was studying with GOMEZ ORTEGA. He "devoured what books he could get" and classified what plants came his way. To GOMEZ ORTEGA's astonishment, he petitioned for admission to the examination for pharmacists; this request of an unknown student was so unprecedented that it was granted, and, moreover, the petitioner passed the examination brilliantly, receiving the title of *farmacêutico*. GOMEZ ORTEGA then took him into his classes; he became the favorite pupil, the companion in excursions, and the intimate friend of the master. He was accorded the honor of giving the lecture with which the course opened; this was printed, and a copy was specially autographed for the author by the royal hand of CHARLES III himself. Shortly afterwards he competed successfully for the post of pharmacist (*boticario mayor*) at the general hospital of Madrid. From this post he went to Mexico as professor in the new Botanical Garden, whose organization was outlined by GOMEZ ORTEGA.

The Botanical Expedition was formally "incorporated" on 4 August 1787 (AGH 462: I 4-6, 14, 18), which was probably on or near the date of the arrival of CERVANTES and his colleagues in Mexico, where they met SESSÉ. (CASTILLO reached the city over a year later, on 14 August 1788; AGH 462: I 17). The first thing to do was of course to find quarters—a site for the garden, a hall for instruction, living accommodations for the staff. SESSÉ's eyes first rested on the ancient Jesuit college of Saints Peter and Paul; at his request the keys were delivered to him on 14 September 1787 (AGH 460: 284. 462: Ib 42-53), and presumably this was the first home of the Expedition. SESSÉ's idea was probably that one building should house the staff and provide for their scientific and academic work. Unfortunately the garden of the college had already been given (in 1770) to the Seminary of San Carlos de Naturales. The old building could be only a temporary resting-place, and was in any case suitable only for classwork.

Then SESSÉ or one of his friends discovered a bit of land which he

[8] AGH 462: I 10. According to LEÓN (Bibl. Bot. Mex. 237) SENSEVE was a native of Mexico. Since he was supporting a wife, Doña MARÍA VICENTE CAMPILLOS, in Spain (AGH 461: III 5), and returned to Spain with the others in 1803, this is dubious.

[9] According to LEÓN (Bibl. Bot. Mex. 88), in Placencia, Cáceres; he also has the date as 1759. He perhaps followed BERISTAIN (Bibliot. Hisp.-Am. 1: 336, 337), who said CERVANTES was "natural del Obispado de Placencia en Extremadura." I have followed GARCÍA RAMOS (Bol. Soc. Mex. Geog. Estad. II. 1: 753-765), who said that he had obtained his information from a member of the family of CERVANTES. LEÓN and BERISTAIN are certainly wrong about the date of CERVANTES' migration to Mexico, which they give as 1786.


PUENTE DE ALVARADO
[AV. HIDALGO]
ALAMEDA
HOSPITAL DE S. ANDRES
[AV. DONCELES]
[AV. TACUBA]
C. DEL RELOX
CATEDRAL
PALACIO REAL
PLAZA MAYOR
JRB
C. DL LA ACEQUIA
PLAZUELA DEL VOLADOR
[AV. MADERO]
[AV. JUAREZ]
ACORDADA
FABRICIO DE CIGAROS
C. DE REVILLA GIGEDO
MARTINEZ
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[AV. S. JUAN DE LETRAN]
C. DEL ZAPO
C. DE VICTORIA
[PASEO DE LA REFORMA]
POTRERO DE ATLAMPA
YC
[AV. MORELOS]
PASEO DE BUCARELI
CAPA DE LA CANDELARIA
LA CIUDADELA
COLEGIO DE S. MIGUEL
C. DEL SALTO DEL AGUA
[ARCOS DE BELEM]
TERRENOS PANTANOSOS
PASEO
CALZADA Y CANERIA DE CHAPULTEPEC
[AV.]
MEXICO IN 1830
after GARCÍA CONDE
Names in brackets are modern
Broken lines represent canals
YC = House of YGNACIO CASTERA
JRB = JARDIN REAL BOTANICO

thought would be suitable for a botanical garden; water was abundant, rock was available, the location was convenient. This was the pasture (*potrero*) of Atlampa. It was situated at the limits of the city, and embraced "all the region between the new aqueduct of the Salto del Agua, various suburbs of the Capital, and the magnificent Paseo de Bucareli."[10] The "suburbs" are fixed by other references as the Colegio de San Miguel de Belém, the Candelaria de los Ciegos, and the Carcel de la Acordada.[11] The first of these may be located on the accompanying map, which was published 40 years later; the street of the same name is now the Arcos de Belém. The prison of the Acordada was situated on what is now Avenida de Juarez, not far from the well known Bronze Horse. The Candelaria was doubtless not far from the Capitania de la Candelaria of the map. The line thus established corresponds roughly to the Calle de Revillagigedo, which was opened through a collection of mean hovels by the Viceroy of the same name; his administration began in 1789. Traces of the "new aqueduct" may still be seen on the Avenida de Chapultepec. It is curious that the Ciudadela is nowhere mentioned as a landmark, though it was within the area outlined by the above boundaries. At all events, use of the Potrero was graciously ceded by the city to the Crown in 1788 for the development of the Botanical Garden.[12]

On 3 October 1787 SESSÉ asked that an architect be commissioned by the Royal Treasury to draw up plans for a building; this should contain a hall for instruction, space for a herbarium, a museum (*gavinete*), and a library; and living quarters for the professor and his staff. Don MIGUEL COSTANZÓ (or CONSTANSÓ) was appointed to make an estimate of the cost of such a building. He presented his estimate in June of the following year (AGH 460: 285. 461: I 37. 462: Ib 42-53). The garden, he said, would cost 83,000 pesos to construct. SESSÉ and CERVANTES replied that this could be cut in half by eliminating the expense of walls of masonry; trees would do as well, the soil being firm. A new estimate was accordingly submitted in September. Not including a greenhouse (651 pesos), the cost of construction was put at 46,060 pesos. The building would cost 21,636 pesos 5 reales—"una casa con vivienda de moderada extension para el catedratico de Botanica, con una aula en que se dén las lecciones publicas de este ciencia."

While these negotiations were thus protracted, SESSÉ and CERVANTES had not been idle; instruction in botany and work on the garden had already begun. On the Calle de Victoria, with its entrance at Bucareli No. 9, was a house belonging to Don YGNACIO CASTERA, "Arquitecto Mayor de esta nobilisima ciudad"; this was turned over, with its garden, to the Botanical Garden by its owner "como leal vasallo y ciudadano noble" (AGH 461: I 27, 28. 466: VIII 13. Gaz. Mex. 3: 77 (supl.) 6 My 1788). In view of subsequent developments, it appears that CASTERA wanted to get rid of his property, and saw a chance of disposing of it; for he offered it, at a valuation of 35,000 pesos, for purchase by the government. Later he revised his

[10] AGH 462: Ib 42-53. "toda la extension que hay entre la Arqueria nueba del Salto del Agua, barios suburbios de esta Capital, y el magnifico paseo de Bucareli."

[11] AGH 462: Ib 4-8, 98-102. Gaz. Mex. 3: 75 (supl.). 6 My 1788.

[12] AGH 466: VIII 15. Gaz. Mex. *l.c.* According to GALINDO Y VILLA (Hist. Sum. Mex. 171), the aqueduct which ended in the Salto del Agua was completed 20 March 1779. It was 4663 yards long and carried by 904 arches.

figure to 31,600; COSTANZÓ pared this down to 29,021 pesos, with 5000-6000 additional needed for converting it to the uses of botany.

SESSÉ and CERVANTES had meanwhile been busy preparing an estimate (dated 17 August 1788) of the cost of running the garden for the first year (AGH 461: I 11, 12). The items of this document give us a picture of what such an undertaking involved in that day. The professor drew a salary of 1000 pesos with an allowance of 800 pesos for a house; besides him the Garden was to be staffed with a head gardener at 600 pesos, first and second assistant gardeners at 400 and 300 pesos respectively, 12 peones at 150 pesos each, and 4 apprentices to learn the arts of "dibujo, jardineria, &a." at 100 pesos each: a total of 5300 pesos for salaries and wages. For flower-pots and tools 200 pesos were set down, and a like sum for the purchase and transport of plants. Other items were as follows. Prizes for outstanding pupils, 200 pesos. Music, printing of dissertations, invitations for the *acto público* ("commencement exercises"), 200 pesos. A doctor and surgeon, to observe effects of medicinal plants in a hospital, 600 pesos. Festival of the patron saint with sermon, etc., 200 pesos. Books for the library, 200 pesos. Librarian, a pupil of the Garden, 200 pesos. Excursions made by the professor with his best pupils to form a herbarium and seed collection, 400 pesos. The grand total for instituting the Garden and running it for a year was 8000 pesos, which included the cost of labor but not the salaries of the staff (nor, of course, the salaries of the other members of the Botanical Expedition).

One feature—a very characteristic one—of the Royal Order in which the Garden was chartered has not been so far noticed: means had to be found to render the institution self-supporting and to reimburse the Crown for the cost of construction. SESSÉ in accepting the Directorship was expected to submit proposals to that end; and did so, in the same long letter which contains his estimates of expenses. His proposals (*arbitrios*) evidence some ingenuity, besides illuminating aspects of the life of the time. They were as follows. (*1*) *A plaza de toros* was to be constructed and the profits turned over to the Garden. SESSÉ estimated the cost of construction at 70,500 pesos, the cost of operation for a year at 8196 pesos, and the revenue from 12 *corridas* at 56,358 pesos; those who are attached to this ancient sport will find much of interest in the details of these estimates. The bull ring, which was to seat 15,000 or more persons, was to be situated between the Paseo de Bucareli and the Calle de Victoria, near the center of the city and convenient for the throngs of carriages and of pedestrians. (*2*) The state lottery was to be run for the benefit of the Garden. (*3*) The Garden was to furnish (during the survey of the natural history of the country) "visitors" to the *boticas* or drugshops, to reform some of the current abuses of the laws regulating their traffic. For this service each *botica* was to pay 50 pesos every two years. Elsewhere SESSÉ gives a list of the *boticas* by cities (AGH 461: I 14). Mexico had 34, Puebla 11, Oaxaca and Vera Cruz 5 each, the others mostly from 1 to 3. (*4*) The University, since it was enjoying rents of greatly increased value, could support the chair of botany. (*5*) Vagrants, drunkards, and other delinquents ("vagos, mal entretenidos, ebrios, y otras especies de delinquentes") were to be used to carry manure and rock, to dig tanks, and to perform other manual work, which would not only ensure cheap labor but would tend to correct these vices so prejudicial to the state.

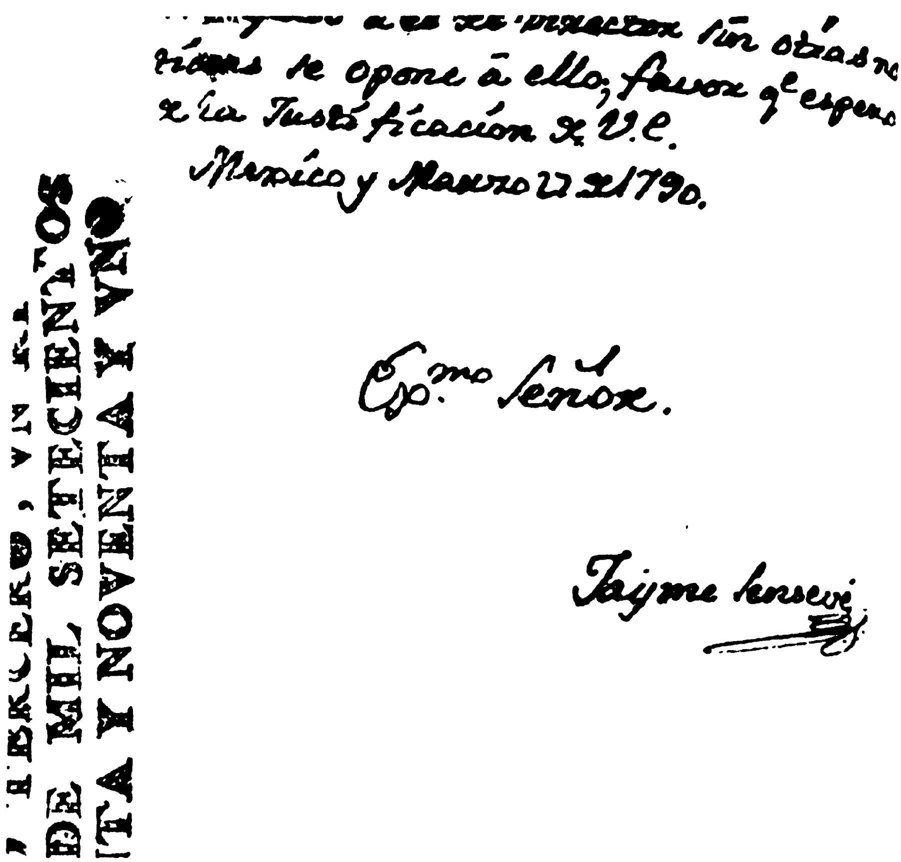
Alas for the brave hopes so confidently embodied in these plans! A

voluminous correspondence attests the passage of the *arbitrios* from official to official, to the Dean of the University, to the *Protomedicato,* to various architects and engineers, to accountants and attorneys, accompanied by an ever swelling mass of covering letters, requests for information, for approval, for additional copies. On October 21 COSTANZÓ presented plans and a model for the *plaza de toros,* with the comment that such a worthy object should not have been allowed to languish; it had apparently been proposed before, under an earlier administration. SESSÉ, he said, had underestimated the cost of construction, which would run to 150,000 pesos. Moreover, as the Attorney General (*Fiscal*) pointed out, the profits of such an establishment were preoccupied for the construction of the palace at Chapultepec (of which more later). One by one all SESSÉ's ingenious proposals came to grief. The Royal College of Physicians (*tribunal del protomedicato*) said that most *boticas* could not afford to pay 50 pesos biannually; if they did they would add it to the price of their goods, so that the public would suffer. (This unnatural concern for the public may direct our attention to the real trouble, the encroachment upon the prestige and the profits of the *Protomedicato.*) The University had no money, could not act. (We shall see something later of its bitter hostility to the new institution of learning.) The labor of vagrants was of small significance and impermanent. (Beggars have never been lacking in Mexico.) Profits of the lottery were already sequestered by His Majesty for the hospitals. COSTANZÓ, summing up the *arbitrios* in the following year, said that they were so impractical, and were based on such weak foundations, that the *Fiscal* and the *Junta* called to consider them had allowed them to lapse as "inadoptables, y futiles" (AGH 462: Ib 42-53). On 29 April 1789, when the Royal will was solicited, the Garden was no nearer financial independence.

Fortunately, SESSÉ and CERVANTES had not waited to begin work until the problem of maintenance was solved, buildings provided, or even the site officially approved and purchased. CASTERA had, as I have said, lent his houses; classes were held in some sort of "aula," probably beneath the arches of a patio (AGH 460: 269). Work on the garden was begun at the same time as classes were opened, in May 1788 (AGH 466: VIII 15). Both the small garden belonging to the house and the adjacent *potrero*—an area of 20,000 square yards (AGH 464: IV 1)—were cultivated; the latter was surrounded by a picket fence and the earth tilled and thrown up in raised beds edged with boards in a common European fashion. Pipes were laid and troughs made for the distribution of water. The whole cost was 1781 pesos (AGH 462: Ib 42-53. 464: IV 1). Seeds from Europe were sown, and plants from the country around were brought in and planted (AGH 462: I 1, 8, 9). A hut was built for the gardeners. Lacking appropriations for the purpose, SESSÉ, petitioned for a grant to cover various expenses. On May 6 he was given 2000 pesos, partly for the purchase of flower-pots and for the expense of opening the course (on May 5), partly for the manufacture of field tents for the longer collecting trips which he was planning.

Much of our information of this period is derived from a characteristic controversy over the payment of salaries. This originated—as many similar complaints were to—with JAYME SENSEVE, the pharmacist; a person of weak intellect and timid soul but intent on his rights. Many pages are filled with his long-winded and rambling letters, always worrying about some item due him. The trouble seems to have been that the auditors

did not know when salaries should start, since the organization of the Botanical Expedition was completed only by the arrival of CASTILLO (in August 1788), and it was manifestly impossible to pay salaries to the members of an institution which had not yet been "incorporated"; so, again characteristically, they were not paid. SENSEVE having, at the cost of reams of paper and rivers of ink, won his point, was paid; and immediately started another campaign for the double pay due him for time spent in the field, to which we owe much of what little information we have about these


se opone á ello; favor q.e espero de la Justificación de V.E.
Mexico y Marzo 27 de 1790.
Exmo Señor.
Jayme Senseve

Conclusion of a letter from SENSEVE, 27 March 1790.

first trips (AGH 461: III 8, 9. 462: I 1-18. 460: III 133, 136, 137, 147).

In spite of all the work done on the Garden, CERVANTES could still write at the end of 1790, "I have already given three complete courses in botany, though the Garden has not been established (*sin hallarse formado el Jardin*), having been obliged to search in the surrounding fields and forests for plants for the lessons" (AGH 463: I 2). SESSÉ had begun his exploration of the Valley of Mexico on 1 October 1787. Though the record is silent on all but his troubles, we can imagine the feelings of the botanist in the presence of such a wealth of vegetation, new to him and largely new to science. He was energetic in the exploration of the country around the capital, collecting plants both for use in the classes and for his scientific survey. By the end

of the year they had extended their travels to 6 leagues from the capital; Toluca is mentioned, as well as San Angel and Los Remedios (AGH 462: I 8, 9; Ib 22-25). About 200 plants had been placed in the herbarium and sketched; new species had been described and notes made on species already known. By October 27, he wrote, plants were becoming scarce in the immediate vicinity, and he proposed to extend his collections to "Zacapixtla, Xochistlan," and other places. Lacking salaries, lacking an appropriation for expenses, he was obliged to pay for these trips himself—servants, horses, maintenance. The controversy over salaries continued through the first half of 1788, endangering, to SESSÉ's consternation, the resumption of collecting in the spring of that year. It evoked a particularly disagreeable letter from Spain, suggesting that if the members of the Expedition were paid double for every trip, they would be always running around the country instead of attending to their work in the capital (AGH 462: I 15, 16). The devotion and energy of the scientist contrast strangely with the petty greed and suspicion of the politician—a contrast which is not unknown even in our own enlightened times. However, just claims were finally satisfied, and SENSEVE was temporarily silenced. From this time on, as SESSÉ and his colleagues were more and more occupied with their survey of the Mexican fauna and flora, and as the Botanical Garden began to provide plants for the use of the students, the two aspects of the program began to diverge. Early in 1790 the Expedition left for Michoacán and Sonora, and was gone many months. We shall here confine ourselves to the further history of the Botanical Garden.

CERVANTES did not live at the Garden in 1788; CASTERA's hospitality evidently extended only to the use of his grounds and patios. Some letters of June and July raise the problem of a suitable home for the *catedrático,* an "avitacion en las cercanias del Jardin (AGH 461: III 146, 148). Since during the rainy season the waters sometimes rose to a depth which made it impossible for students to reach their classes (and of this more later), the desirability of installing the professor on the grounds is evident.

A clear picture of the work of the Botanical Garden during this first year, especially of the items involved in its administration, is given in the accounts rendered by CERVANTES.[13] The largest single item, exclusive of salaries and wages, was the purchase of 1597 flower-pots of various sizes and prices, totaling 182 pesos (not far from the estimated amount). Next in value were the portraits of the Virgin and of His Majesty CHARLES III, obtained for 140 pesos, with 63 pesos 6 reales extra for their transportation. Garden tools (5 spades, 1 pick, 2 hoes, 2 baskets) amounted to 25 pesos 2 reales; a couple of sprinklers cost 2 pesos. Soil was brought from San Agustin de las Cuevas, 166 loads for 59 pesos 7 reales, and 4 canoe-loads of manure for 9 pesos. Tin labels were bought to identify the plants, 700 for 35 pesos. Chairs, benches, curtains, gratings, and other furniture claimed 294 pesos. An interesting item is "the chair" (*la catedra*), which cost 63 pesos 6 reales. What would seem to us perhaps a disproportionate amount of the whole budget was spent on the graduation exercises (*acto público*). Including 400 invitations and 100 handbills distributed through the city, and prizes for the best students, this ceremony ran to 342 pesos

[13] AGH 464: I 1, 2. The accounts are here given in a résumé of expenditures from 11 March 1788 to 31 December 1790. The original documents seem to have been lost by CERVANTES (AGH 465: V 4, 5) "en el reconocimiento de infinitos papeles." See also 465: IX.

4 reales—more than one-fifth of the yearly expenditure. The Garden employed two gardeners, besides, at certain times, various beadles, watchmen, and janitors. The total cost for the year was 1676 pesos 3 reales, exclusive of CERVANTES' salary but including the rent of his house.

In spite of many obstacles, some natural, some political, the course in botany was taught, presumably in accord with the detailed directions supplied by His Majesty and to the satisfaction of the students. The first *acto* was held in the University on December 20—the first of a long procession of annual ceremonies. The graduation of students from a course in botany given by an institution which existed only on paper and for which no financial arrangements had been made must be regarded as something of a feat. The Viceroy was unable to attend, but otherwise the affair was graced by the presence of representatives of the faculty, the church, the military, and the gentry, presided over by the Regent, GAMBOA (AGH 460: 134, 135. 461: II 7, 21. Gaz. Mex. 3 (23): 214. 6 Ja 1789).

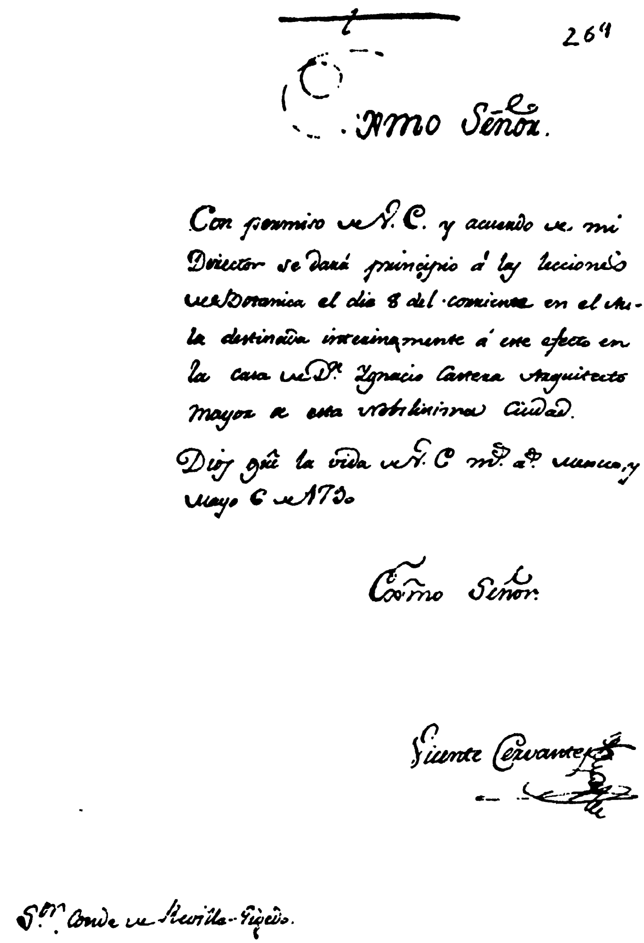
The functions of the Garden were continued through the following year on the same temporary and unsatisfactory basis. (In 1791 it was still referred to as the "provisional botanical garden." AGH 464: VII 15.) The course of 1789 was signalized by the attendance of JOSÉ MARIANO MOCIÑO, later to become associated with the Botanical Expedition and destined to eclipse his master in botanical renown. Cultivation of the Garden was likewise actively continued, as we must infer from the item of 80 canoeloads of manure in the expenses of the year. In the accounts for 1790 we read for the first time of trees being brought into the Garden from San Angel. In January 1791, 5 "canoitas" (canoes or troughs) of wood were made to irrigate the beds. In February an "hazada grande" (plough) was rented for 24 days. *Rosales de Castilla* were brought from San Angel for 7 pesos.

For the first three years the cost of maintaining the Garden amounted to 2854 pesos 5 reales. A letter from COSTANZÓ to the Viceroy in 1791 indicates that this did not include the 1781 pesos expended in construction (AGH 464: IV 1-3). SESSÉ, who signed the statement, said that he had received 2000 pesos towards the amount (this was probably the sum granted him early in 1788, though from this he expected to meet other costs not directly chargeable to operation of the Garden). It was not unusual for His Majesty's trusted servants to have to meet much of the expenses of their duties from their own pockets.

By this time it was becoming apparent that the efforts to establish a garden at Atlampa were futile. The choice of land had been bad. A letter from the new Viceroy, the count of REVILLA-GIGEDO, dated 10 April 1791, takes note of the frequent floods (AGH 462: Ib 2). COSTANZÓ, indeed, had known of this condition at Atlampa; its popular name of "El Sapo" (which accounts for the Calle del Zapo on the map) referring to its constant wetness.[14] This part of the city lay well beyond the ancient limits of Tenochtitlán, where Aztec canoes used to ply the waters of Lake Texcoco; in the map of 1830 all this region, intersected by numerous canals, is still labeled "terrenos pantanosos." In 1943 I walked, not dry-shod, along the Calle de Victoria after a shower and watched the water pouring from the

[14] **I am informed by Dr. M. MALDONADO KOERDELL that this name, "The Toad," is currently applied to an entirely different part of the city, near its northwestern outskirts; doubtless for the same reason.**

street into the doors of the buildings. Since, however, "este accidente es commun a todos los exidos y terrenos que rodean esta capital," it had not occurred to COSTANZÓ to mention it, considering the good location at the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Exmo Señor.

Con permiso de V. E. y acuerdo de mi Director se dará principio á las lecciones de Botanica el dia 8 del corriente en el Aula destinada interinamente á este efecto en la casa de Dn. Ygnacio Castera Arquitecto Mayor de esta nobilisima Ciudad.

Dios gue. la vida de V. E. ms. as. Mexico, y Mayo 6 de 1790.

Excmo Señor.

Vicente Cervantes

Sr. Conde de Revilla-Gigedo.

Announcement of the opening of the course in botany in 1790, by CERVANTES.

"With the permission of Your Excellency and the concurrence of my Director the lessons in botany will be begun on the 8th day of the present month in the hall allotted temporarily for this purpose in the house of Don Ygnacio Castera, Architect-General of this most noble city.

God preserve the life of Your Excellency many years. Mexico, May 6, 1790.

Excellency

VICENTE CERVANTES

Señor Count of Revilla-Gigedo.

limits of the city and its proximity to main thoroughfares (AGH 462: Ib 98-103). One can only wonder if perhaps the good CASTERA was influenced by this condition in his benignity towards the Botanical Garden, and if the choice of the site may not have been brought about through his influence. At all events, in the beds designed to hold certain plants, according to

COSTANZÓ, not even maize would grow because of the "clayey, arid, and nitrous" character of the soil (AGH 462: Ib 13). The unsuitability of the site received confirmation from a new and expert source. This was JACINTO LOPEZ, sent from Spain to be head gardener, who was able to add other particulars from his fourteen years of experience in the Royal Botanical Garden of Madrid. He had arrived probably in 1790 (AGH 462: Ib 13. 464: III 1-6. 466: VIII 15).

Just who first suggested a move does not appear, nor who introduced Chapultepec as a possible site. COSTANZÓ, in a long letter of 13 April 1791, gave his views on the feasibility of such a translation (AGH 462: Ib 4-8; *see also* 54-65). "Among the many uses [he wrote] which have been considered for the Royal Site of Chapultepec, none, in my opinion, suits it better than that of a botanical garden." The Royal Palace of Chapultepec had been started in 1784, when MATÍAS DE GALVEZ was Viceroy. It was energetically continued by BERNARDO DE GALVEZ in 1785 to provide employment in the general distress then prevalent. Up to January 1787, 12,377 pesos had been expended. Most of this had gone into excavation, foundations, and walls. To complete it would have cost as much again. In its then state it was completely useless. It had been planned apparently for the magnificent view over the city; rooms were small, water was lacking. Work on the building had ceased, because of the "condition of the treasury." It had been proposed to sell it, but in its incomplete state it was unsaleable; and the attached land was insufficient for one of the large estates then favored by the Mexican nobility. For a botanical garden, on the other hand, the site seemed ideal, having a variety of aspects and levels, the soil being moist and of good quality—as evidenced by the luxuriance of the native vegetation. It was only a league from the city.

COSTANZÓ suggested that the classwork might continue to be held in CASTERA's house—since it had been objected that even a league was too far for students to go for their classes (AGH 462: Ib 12-20). The residence of the *catedrático* might also be fixed there (it is possible that by this time CERVANTES was living there). Meanwhile, however, another idea was being circulated; the contribution of the energetic JUAN VICENTE DE GÜEMES PACHECO DE PADILLA, second Count of REVILLA-GIGEDO and fifty-second Viceroy of New Spain, who had succeeded MANUEL ANTONIO FLOREZ in 1789. Attached to the Palacio Real, in which he had his official residence, was a small garden, with a gallery or arcade along one side which would do for an *aula*. Inexpensive houses were available in the vicinity for the members of the staff. Here might be grown the plants for the use of students and for the delectation of the public, while larger plants, and a greater variety, could be grown at Chapultepec and brought in as needed. CERVANTES (acting as Director while SESSÉ was in the field) and LOPEZ were invited to comment; both expressed approval (AGH 462: Ib 22-25). CERVANTES pointed out that such a plan would enable him to offer classes in medicine, surgery, and pharmacy, hitherto impossible because of the inaccessibility of CASTERA's house. The garden of the Palacio, he said, would hold about 1000 species, enough for class use; moreover the students could easily visit Chapultepec when the weather was fine. Finally, COSTANZÓ, reviewing this solution of the problem, pronounced it, in his pompous way, "advantageous to the Treasury, very useful in the establishment of the School of Botany, and convenient for the public of this capital (AGH 462: Ib 26).

CERVANTES asked only that the gallery be covered with glass to keep out the rain and to afford protection from the sun, since it faced east; and that the raised walks, being too low and badly constructed, be removed and rebuilt. He said, moreover, that the soil, which was "pure gravel and prejudicial to almost all plants," should be excavated to the depth of a yard and replaced with soil chosen by LOPEZ in Tlaspana (AGH 464: VI 1). Elsewhere he hopes that the *aula* will be finished by mid-December (1791); which implies that the changes he requested were being made (AGH 463: I 6).

An estimate of the cost of removal, including alterations, was submitted on October 2. For 1607 pesos, apparently, it would be possible to bring in 6000 loads of soil and 7500 bricks, 12 cartloads of lime and 200 loads of sand, and tiles enough to recover the walls, besides having enough for tools and for the hire of masons and laborers. The job was actually done for 1556 pesos, by Capitán de Ingenieros Don MANUEL AGUSTÍN MASCARÓ. The order for removal was dated September 28; the moving was done from October 3 to October 8, and the seven following weeks were occupied in the necessary alterations and construction (AGH 464: VII 2, 10, 15).

So after nearly four years of proposals and counterproposals, of vain attempts to grow plants in the alkaline clay of Atlampa and to conduct classes in a suburb which was under water in the rainy season, "El Sapo" was deserted; and the Royal Botanical Garden found what was to be its permanent home in the Palacio Real on the Zocalo, where Aztec priests had once torn the living hearts from their thousands of victims and tumbled their quivering bodies down the steps of the great pyramid.

CASTERA, cheated of his ambition to sell his house, was not wholly a loser. On 20 June 1792, a final payment was made him of 1076 pesos 1 tomin 6 granos (AGH 460: 251, 253). Atlampa was leased to one Don JOSÉ DE HEREDIA, purveyor of beasts of burden to the court, by what legal subterfuges it is not now clear; the land from the beginning having been involved in litigation with some Indians over a sum of about 140 pesos. (AGH 462: Ib 4-8, 42-53). HEREDIA now insisted that it be promptly vacated, so that he might sow alfalfa for the beasts in his care. The business dragged on until CERVANTES had to write assuring the Viceroy that the land had been entirely vacated as soon as the order to move had been received. They ought, he added, instead of paying the back rent for which HEREDIA was clamoring, charge him for the fence around the property, or move it to Chapultepec (AGH 463: V).

The garden in the Palacio Real became, as I have said, the permanent home of the Botanical Garden. Here, from 1791 until the institution ceased to exist some time after 1820, CERVANTES taught those courses in botany which became almost requisite to a well rounded education in Mexico.

For some time after the move, negotiations were continued to obtain the site at Chapultepec for the uses of the Garden. But a letter from ARANJUEZ, royal minister in Spain, must have rather dampened their enthusiasms (28 April 1792; AGH 462. Ib 74-77). It was a thoroughly nasty letter, abounding in insinuations against the integrity and intelligence of every one concerned. "The king having gone into the matter [wrote ARANJUEZ], it has seemed to His Majesty very unlikely that the Potrero de Atlampa has lost, since 1789, those advantages which led to its choice as the site of the Botanical Garden by the Director of the Botanical Expedi-

tion and of the said Garden, Don MARTIN SESSÉ, by the Professor of Botany, Don VICENTE CERVANTES, by this same COSTANZÓ [who was now writing on behalf of Chapultepec], by the attorneys, and by your predecessor in office; and no less unlikely that it has not until now been discovered that the place is swampy, this being an easily apparent quality." Moreover, he continued, if Chapultepec were unsuitable for a palace, it must be also unsuitable for a botanical garden! "In consideration of all which, His Majesty has resolved that the Botanical Garden should be continued in the Potrero de Atlampa," measures being taken to prevent floods. His Majesty's ignorance of actual conditions in Mexico is as obvious as his opinion of the honesty of his servants. No further commentary is necessary on the discernment of his ministers. It must be remembered that we are no longer dealing with the enlightened CHARLES III, but with the corrupt and entirely contemptible court of CHARLES IV.

The decision to move the Garden and to divide it between the two sites, and the actual transfer from Atlampa, had been made during SESSÉ's extended journeys in western Mexico of 1790-1792. When he returned to the capital, one of his first acts was to examine the proposed site at Chapultepec. Previously [he wrote, in a letter of 20 July 1792; AGH 462: Ib 84-95] he had known only the swampy corner which is seen "to the right of the entrance, in the left-hand part," and had feared the same trouble as with Atlampa. But now having gone over it more carefully with CERVANTES and LOPEZ, having seen the wide region between the ditch called "las orcasitas" behind the wood and the new powder factory, having remarked the ease with which this could be turned into a sloping plain, having noticed on the north the trench which brings water from Santa Fé,—he was ready to be convinced of the advantages of the place. Not only would it support the same plants as then grew in Atlampa, but also the mount (*cerro*) permitted the naturalization there of succulents, African, Alpine, and others, adapted to dry and rocky places, without any cost for irrigation. The part facing north was well fitted for these, being naturally covered with them; the part facing south would be suitable for plants of warmer soils; the talus slope, sheltered by the mount and by the aqueduct on the north, harbored species of all temperaments and gave an idea of what would prosper there without effort; finally the corner by the entrance was suited for aquatics and for those peppery plants (*plantas acres y piperitas*) so abundant in the Americas and so valuable in medicine and commerce.

Of the small garden in the Palacio Real he approved no less, for teaching. His estimate of the number of species that could be cultivated there was 2000, "with plenty of room, that is, six or eight feet for each species." (In the most flourishing days of the Garden, 1400 species were grown there.) The only bad feature of this garden, he thought, was that it deprived the public of the beauty and recreation which they would get from a garden on a principal avenue [such as the Paseo de Bucareli]. He suggested that they take down the wall on the side towards the Calle de las Azequias and the Plazuela del Bolador [see the map] and substitute an earthen bank, with a door "que sirviese de entrada principal â todas las personas desentes que se permiten en el articulo 4°. del Reglamento del Jardin." This would beautify the garden, the little square, and the street—at that time one of the most brilliant in the city; it would allow the public to gaze upon "esta preciosisima obra"; it would considerably enlarge the garden; and finally it would provide the ventilation so necessary for the

growth of the plants. At the end of the gallery two rooms could be made, one for the library, the other for the herbarium; or these, and others, could be built over the gallery. The quarters of the *Comandante de Ynvalidos* would serve as a residence for CERVANTES.

In a later letter, as he was planning his voyage to Cuba in 1794, SESSÉ mentions that CERVANTES is to direct the planting of the garden at Chapultepec, which was to grow "las plantas de los diversos climas que componen esta America" (AGH 460: 230-232). The palace garden, meanwhile, was to continue to serve for teaching, and "to offer to the Capital its most beautiful spectacle, visible from its principal square if it were given the extension of which it is susceptible." Probably, therefore, these plans were never carried out (if they had not been by REVILLA-GIGEDO, who from the first had interested himself in the development of the Garden, they certainly were not by his successor, the avaricious and detested BRANCIFORTE). The Garden doubtless continued to be separated from the Zocalo by the main structure of the palace, and from the street to the south by a wall, much as its small modern relic is.

In 1792 COSTANZÓ reviewed the entire problem and confirmed SESSÉ's description (letter of 30 July; AGH 462: Ib 98-102). He went over the impossibility of selling Chapultepec as a *hacienda de campo,* the area being only about 220,000 square yards, instead of the minimum of 10 square leagues needed for this purpose. He mentioned several "corpulent" trees called *ahuehuetes*[15] growing between the mount and the aqueduct, and outlined a simple scheme for bringing water in a pipe resting on three beams, each 6 or 8 yards long, from the aqueduct to the vicinity of these trees, whence it could be conducted entirely around the mount in a wooden conduit. It had been suggested, incidentally, that the palace house the *Archivo General* (AGH 462: Ib 54-65).

The Attorney General took a sympathetic attitude (in his letter of 16 September 1792; AGH 462: Ib 104-110); particularly since the Atlampa field had already been designated as the site of the new cigar factory. It was [he wrote] as difficult to find a place for this as to find a use for Chapultepec; the latter had been nothing but a drain on the treasury, which after many years had resulted in nothing but "la ruina de un edificio inhavitado."[16] Of the cigar factory he said: "it borders on inhumanity how through many years the great multitude of men and women employed in it has been maintained, in hovels exposed to the inclemencies of the weather .... whence come the illnesses of these workers in an industry which has brought many millions of pesos into the Royal Treasury." For such reasons—and we hope he meant the conditions which he described as well as the revenue at stake—the cigar factory had prior rights over the Botanical Garden. It appeared that His Majesty, though he had forbidden the use of Chapultepec for the latter institution, had not mentioned the garden of the Palacio Real, in which instruction might be (and was) continued even though the rest of the plan had to be jettisoned. Indeed, even the Royal prohibition seems not to have been taken too seriously; some attempts at planting Chapultepec

[15] BULLOCK (Trav. Mex. 154) saw these "immense cypresses" (*Taxodium mucronatum*) in 1823; he described them as 60 feet in girth.

[16] BULLOCK (Trav. Mex. 154) characterized it in 1823 as unoccupied and dilapidated. For the first mention of the cigar factory, see the letter of REVILLA-GIGEDO in AGH 462: Ib 78.

were still being made in 1794.[17] The matter was still worrying His Majesty's ministers in Spain in 1800. In that year ARANJUEZ acknowledged the Viceroy's reports and requested further details on the two sites, wanting to know how it happened that Atlampa was turned over to the cigar factory. An imposing file testifies that the ball was kept rolling from the *Fiscal de lo Civil,* to the *Director de Arquitectura,* to SESSÉ and CERVANTES, back to the *Fiscal,* thence back to the Viceroy, by this time His Excellency Señor Don JOSÉ DE YTURRAGARAY, whom it was still bothering in 1807.[18] It is impossible to say now whether or not His Majesty would ever have found his way through this quagmire of documents to a final decision; NAPOLEON intervened, and the sword proved mightier than the pen.

What a pity these men did not succeed in establishing a botanical garden in Chapultepec! The old *ahuehuetes* are gone, but one can now walk past their successors into a fine park, which supports a luxuriant and varied vegetation; the mount is crowned by MAXIMILIAN's imposing palace, now a historical museum. It is true the park holds the *Instituto de Biología* of the National University; but little teaching is carried on here, and no botanical garden is maintained. To be sure, also, there is beyond the "zoo" a small *jardín botánico;* but this was never planned on a scale nor with ideals which could be compared with those of the garden which SESSÉ and CERVANTES had in mind.

[17] AGH 464: XIX. 465: II. Also SESSÉ's letter already cited in 460: 232.

[18] Letter of 20 June 1800, and accompanying papers. AGH 462: Ib 158-173. For the location of the *fabrica de cigarros* see the map. The first edition (1793) of this map is reproduced by BULLOCK (Trav. Mex.); on it the building usually known as La Ciudadela is labeled "R.[1] Fabrica de puros y cigarros." Apparently the necessity of moving the cigar factory from this location led to the controversy over a new site for it.

Chapter II

MOCIÑO: EXPLORATION OF MEXICO

On April 30, 1787, the Royal and Pontifical University of Mexico conferred the degree of Bachelor of Medicine upon "JOSÉ MARIANO MOZIÑO SUARES LOSADA, natural de Temascaltepec." This young man, in that year only 30 years old, had already distinguished himself as a student and professor of theology, philosophy, and history. He had married nine years before. Two years later we find him studying botany in the Royal Botanical Garden of Mexico; and the next year his wife sued for divorce.

"Jph. Mariano Español," legitimate son of the legitimate union of Don JUAN ANTONIO MOSIÑO and Doña MANUELA LOSADA, was solemnly baptized on September 24, 1757. The ceremony occurred in Temascaltepec, in what is now the State of Mexico, where his parents were resident. Though politically Mexican, the boy was of pure Spanish descent, his parents being "cristianos biejos, sin mexcla en la sangre, ni infamia en linaje."[19]

Poverty was his portion from the beginning. We may picture him, a country lad of 17, scion of a respectable but somewhat decayed family, arriving in the capital of New Spain, glancing, as he picked his way through the filthy streets, into the flowery patios of splendid houses of stone and tile, gazing round-eyed at the magnificent churches, at the hundreds of colleges and convents, watching the crowds of Indians driving their slim canoes along the muddy canals, catching on every side the glitter of wealth and the echoes of a great European monarchy. He was an alert youth, apt at his studies; he wanted to be a scholar. He came seeking an allowance or scholarship (*ración*) to permit him to study in the Seminario Tridentino. An applicant had to present evidence of his legitimacy and of the purity of his life (*legitimidad y limpieza de vida y costumbres*) ; to gain his allowance MOCIÑO also submitted a statement of the inability of his parents to support him as a paying student. Finally the Rector certified that the applicant was notorious for his application, his poverty, for progress in his studies, and for good manners. The *ración* was granted; MOCIÑO could assume the gown and tippet, *el manto y la beca,* of a collegiate in the seminary.

During his years as a student of the seminary (1774-1778) his teachers could congratulate themselves. A contemporary biographer speaks of him as outstanding among his fellow-students for his extraordinary talents (BERISTAIN, Bibliot. Hisp.-Am. 2: 353). He was the perfect student, "dedicated to learning from his childhood" (Dicc. Univ. Hist. Geog. 5: 582). Having graduated in philosophy in 1776 (the appropriate public exhibition occurred on March 22 of that year), and subsequently in scholastic theology and ethics, in 1778 he was examined in "todo el tom. de Mysteriis, et annis Xpti. del P. M. Graveson" in order to evidence his vast erudition, which he accomplished "to the satisfaction of the whole college, with general applause" (quoted by CARREÑO, Not. Nutka xi). These de-

[19] "Christians of long standing, without mixed blood or stained lineage." A copy of the birth certificate was made October 15, 1773; this document and others attesting to his legitimacy and good manners are preserved in the archives of the University of Mexico, and are cited by CARREÑO, Not. Nutka vi-viii.

tails are drawn from the annual reports on the "charity students" who supported themselves on the leavings (*sobras*) in the refectory of the seminary; which affords us some insight into the character of the *ración* allowed by a generous faculty.[20]

At the age of 21, then, José Mociño was one of the most promising young scholars in Mexico, and could look forward to a distinguished career in the church or in the university. At this point, says Carreño, the illusions of the Rector and the Faculty were shattered by the entrance upon the scene of the god of love.

The *ración* which Mociño had been granted in 1774 was obtained through the intermediacy of his uncle, Dr. Don Luis de los Ríos. It appears that Don Luis lodged in the house of Doña María Rita Rivera y Melo Montaño, in which house Mociño also was received as a guest.[21] The prospects of a clerical career were no match for the charms of Doña María, seen thus intimately. Mociño himself was gay and lively; the lady yielded to his ardent solicitation. Just when they were married does not appear. From the proceedings of 1790 mentioned below, it seems that it was in 1777. In the following year the next act of the comedy began.

In 1778 the new Archbishop of Oaxaca took with him to that sleepy little semitropical city Don Luis de los Ríos as official theologian (*teólogo de cámara*); with him went the brilliant young philosopher Don José Mociño, and doubtless his bride (Not. Nutka xii). In Oaxaca Mociño was "Catedratico Propietario de Filosofia en el Seminario de esta Sta. Yglesia," and from 1779 to 1783 gave lessons in ecclesiastical history "con la critica relativa â los hechos controvertidos" (AGH 464: XV 1). He also at times discharged the duties of the chairs of theology and ethics (Dicc. Univ. Hist. Geog. 5: 582).

Ramírez conjectures that the young professor soon became disgusted with the sterile sophistry of scholastic philosophy and turned his eager mind to the study of medicine and the natural sciences (Sessé & Moc. Fl. Mex. vi). In fact he joined Alzate in open hostility to scholasticism, expressing himself, in the *Gaceta de Literatura,* "con los punzantes epigramas que le inspiraba su carácter festivo." His earliest published work seems to have been something of this kind. Beristain (Bibliot. Hisp.-Am. 353) mentions by title two pamphlets, and says that he published, under the name of D. José Velasquez, "varias cartas y satiras contra los Aristotélicos, y escolásticos de mal gusto." In this passing phase of his career we can

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[20] In 1793, when Mociño was applying for a place in the Royal Botanical Expedition, he submitted a record of his education, with attesting documents. These documents are preserved in AGH 464: XV 1-13.

The several forms of Mociño's name used above reflect a freedom in spelling characteristic of the time. In the official testimonials just cited the student was referred to as "el Br. Dn. Josef Mariano Mosiño Suarez y Losada," as "Dn. Josef Mariano Moziño Suarez de Figueroa," and as "Dn. Josef Mariano Moziño Suarez de Figueroa y Losada." In some correspondence of 1793 (AGH 462: Ia 47-54), he is regularly named Musiño by Jayme Senseve, Moziño by Martin Sessé, and Mociño by the Conde de Revilla-Gigedo. Losada was his mother's name. The significance of the "Suarez de Figueroa" I do not know. The variants of Mociño, of course, sounded much the same when spoken. It is rather unfortunate that this form should have been adopted as more or less official, for the owner of the name always signed it Moziño, and so it was written also by Sessé; this spelling is adopted also by Mociño's biographer Alberto M. Carreño. It would be difficult, however, to insist on a change now.

[21] Carreño, Not. Nutka xi, xii, quoting the *Archivo General.*

"MIL CUMBRES" ON THE BORDERS OF MICHOACÁN.—This is the country which the Expedition penetrated on its journey of 1790-1792.

THE VOLCANO "PARÍCUTI" (MICHOACÁN) in 1943. — The actual cone is hidden by the cloud of ash; several older and now inactive cones may be seen. Near this spot Jorullo was in eruption in 1790, and doubtless presented much the same aspect.

discern a young man of considerable character, of wit and gaiety as well as scholastic aptitude.

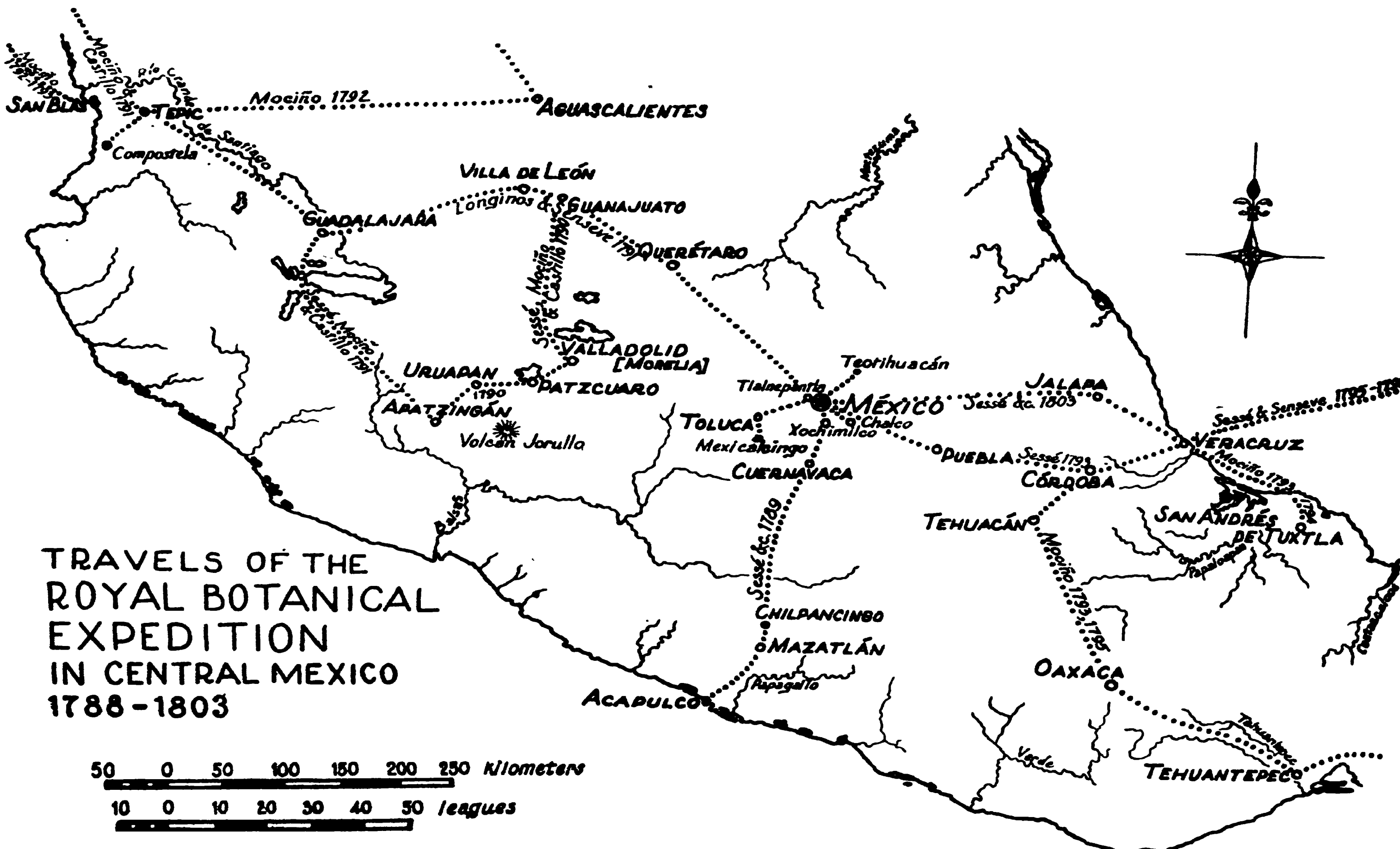
As professor of philosophy in a small provincial college, MOCIÑO was what we call a failure; but by such failures the world is the richer. That inquiring and independent intelligence had to escape, or wither in those dry pastures. It was unfortunate that he was married to a woman who could not share his enthusiasms and ambitions. Doña MARÍA, however, was not wholly to blame for the estrangement which arose between them. She was a creature of her environment, in which the proprieties counted for much. Probably also, like most women, she resented anything that cast a shadow on the economics of married life. Where was the sense in forsaking a good position, abandoning the bright promise of early promotion, in short jettisoning a "successful" career, for the further years of privation and study which her malcontent was now thinking of? Perhaps also she found herself neglected as her husband's new interests took him abroad into mountains and forests. Bishop JOSÉ GREGORIO of Oaxaca took her side, and spoke of MOCIÑO as a misguided young man, without property, without means other than the great talents and culture with which God had endowed him, and said that he had always contemplated this marriage with very great regret.[22] On MOCIÑO's behalf we may adduce a certain whining, not to say nagging note which is apparent in his wife's letters, and which may suggest a personal reason for his disconent with life in Oaxaca. She deposed, for instance, that her husband, "after having made use of my poor house to the utmost of his ability for seven years without troubling to exert himself to provide me with the necessary sustenance, went to Mexico to graduate in the faculty of medicine leaving me abandoned and absolutely without support."[23] MOCIÑO said later that she left him of her own accord, meaning presumably that she would not come back with him to Mexico.

In 1784, then, behold our youthful philosopher and theologian once more enrolled as a student. The years have not dulled the keen edge of his abilities nor quenched his zest for learning. In less than three years he successfully completed the four courses in medicine and could style himself a *Bachiller*.[24] His biographers generally say that he studied physics, mathematics, botany, chemistry, and anatomy (BERISTAIN, Bibliot. Hisp.-Am. 353). It is asserted that he was "sustituto de matemáticas" in the University, and instructed in botany for an entire year; also that he obtained the title of visitor for the *tribunal del protomedicato;* and that *in consequence of this* he made extensive journeys in Mexico (Dicc. Univ. Hist. Geog. 5: 582). We know that his journeys had another origin; and in general all these extravagant claims seem unfounded. MOCIÑO had a course in mathematics in 1786, taught by MIGUEL COSTANZÓ (AGH 464: XV 12); but to make him master of all the sciences BERISTAIN must have fused the persons and attainments of several of the distinguished young

[22] Letter of 14 August 1790. AGH 465: XI 31. "Un descaminado, sin abrigo, ni otro arrimo qe. su gran talento, y buenas letras, de qe. Dios lo ha dotado . . . Siempre he mirado este matrimonio con mui particular lastima."

[23] AGH 465: XI 31. "mi marido, despues de haver usufructuado mi pobre casa en quanto pudó, durante siete años sin cuidar de esforsarse â darme los alimentos necesarios; se retiro à Mexico â graduarse en la facultad de medicina dexandome abandonada sin proverme absolutamente de mis asistencias."

[24] Documents relating to MOCIÑO's medical training are preserved in the archives of the University and are cited by CARREÑO, Not. Nutka xiii-xv.


TRAVELS OF THE
ROYAL BOTANICAL
EXPEDITION
IN CENTRAL MEXICO
1788-1803
SAN BLAS
TEPIC
Compostela
Río Grande de Santiago
Mociño & Castillo 1791
Mociño 1792
AGUASCALIENTES
VILLA DE LEÓN
GUANAJUATO
Longinos & Sessé 1791
GUADALAJARA
QUERÉTARO
Sessé, Mociño & Castillo 1791
Sessé, Mociño & Castillo 1790
VALLADOLID
[MORELIA]
URUAPAN
1790
PATZCUARO
APATZINGÁN
Volcán Jorulla
Teotihuacán
Tlalnepantla
MÉXICO
TOLUCA
Chalco
Xochimilco
Mexicalcingo
CUERNAVACA
Sessé &c. 1789
CHILPANCINGO
MAZATLÁN
Papagallo
ACAPULCO
JALAPA
Sessé &c. 1803
PUEBLA
Sessé 1793
CÓRDOBA
TEHUACÁN
Mociño 1793, 1795
VERACRUZ
Sessé & Senseve 1795-1798
Mociño 1793
SAN ANDRÉS
DE TUXTLA
Papaloapan
OAXACA
Verde
Tehuantepec
TEHUANTEPEC
Balsas
50 0 50 100 150 200 250 Kilometers
10 0 10 20 30 40 50 leagues

Mexican scientists of that period. He enrolled in CERVANTES' second course, which opened on 4 May 1789[25]; and graduated on November 21 of the same year. According to the *Gazeta de Mexico* (3: 439. 22 D 1789), MOCIÑO opened the proceedings in November, 1789, with "un elegante discurso," and defended the Linnean system (particularly Linnean nomenclature) against the assembled doctors; concluding by determining and describing the following plants: *Plumiera alba, Loranthus americanus, Serapias mexicana, Bignonia stans.* This event was noticed even in the *Gazeta de Madrid,* which spoke of the "application and aptitude of the natives" [of New Spain]. SESSÉ wrote of MOCIÑO in 1790 as the most outstanding pupil of the school (Not. Nutka xlvi).

This last year of study was decisive in MOCIÑO's career. SESSÉ was having trouble with the Botanical Expedition.[26] The naturalist Don JOSÉ LONGINOS MARTÍNEZ was openly insubordinate; he refused to accompany the others on their travels, would not even work in the common laboratory in Mexico nor communicate to the Director the results of his labors.[27] Don JAYME SENSEVE, pharmacist, was spending much of his time writing those long, rambling, incoherent letters about arrears of pay and other grievances, already mentioned in this work. The same SENSEVE had turned out to be, according to SESSÉ, poorly qualified for the study of natural history, capable only of the mechanical work of dissection (Not. Nukta. xlv. AGH 462: III 32). Elsewhere SESSÉ speaks of his "debil entendimiento" (AGH 462: Ia 34). In place of this lamentable creature, SESSÉ proposed the brilliant young student MOCIÑO. The financial arrangement—always a prime consideration in any undertaking supported by His Majesty's treasury—was of the simplest. Members of the expedition were paid double for time spent in the field. SENSEVE was to busy himself with dissection in the city, continuing to enjoy his basic salary of 1000 pesos a year; MOCIÑO was to accompany the others on their expeditions and receive the extra 1000 pesos which SENSEVE forfeited by staying at home. This scheme was approved by the Viceroy in a letter of 24 March 1790;[28] and on May 17 the expedition—scientists, artists, Indian servants, pack animals, and baggage—set forth for "Mechoacan y Sonora," taking with them the new-fledged botanist JOSÉ MOCIÑO (AGH 462: V 38, 39).

They set out towards the northwest. On the second day of the journey SESSÉ wrote from "Tanepantla" (Tlalnepantla, near Mexico), informing the Viceroy of LONGINOS MARTÍNEZ' refusal to accompany the Expedition (AGH 460: 273). On May 28 he wrote again on the same subject from Querétaro (AGH 460: 271. 462: V 38, 39. 464: X 9). Then they turned west and south; SESSÉ later wrote of MOCIÑO as having spent the year traveling in "las Provincias de Mechoacan, y Nueva Galicia"; the latter corresponding roughly to the modern states of Zacatecas, Jalisco, and San Luis Potosí. By summer they were as far south as Valladolid (now Morelia). On August 9 SESSÉ wrote from this city about one of SENSEVE's

[25] COLMEIRO, Bot. Hisp.-Lusit. 12; LEÓN, Bibl. Bot. Mex. 86.

[26] The Expedition would perhaps be more properly styled the Natural History Expedition (or, as some have more recently called it, the Scientific Expedition), since it included zoologists as well as botanists and was supposed to take account of all the "natural products" of the country, "los tres ramos de Historia Natural." By its contemporaries, however, it was always spoken of as "la Expedición Botánica."

[27] LONGINOS MARTÍNEZ' part in this history is described below (Chapter IV).

[28] AGH 462: V 19; III 33. This is given as the date from which MOCIÑO was employed. However in 462: V 42 the letter of the Viceroy is referred to as of March 21.

interminable complaints, which were still exercising the viceroy in Mexico; and on August 17 from the same place a long letter on another subject (AGH 462: Ia 43, 44. 461: III 15, 17). From Valladolid they turned westward, penetrating the wild volcanic country of the Tarascans of southwestern Michoacán. It was doubtless on this trip that MOCIÑO composed his Latin verses on the still active volcano of Jorullo, verses apparently never published, but referred to by BERISTAIN (Bibliot. Hisp.-Am. 353) and by PABLO DE LA LLAVE,[29] and admired by CASIMIR GOMEZ ORTEGA (Dicc. Univ. Hist. Geog. 5: 583). They passed through Patzcuaro and Uruapan,[30] near the spot where in 1943, long after Jorullo's fires had died down, the fiery heart of that land poured itself out through another vent, and in a few months piled up a cone of lava and ash over a thousand feet high. The modern tourist visiting Parícuti may share the experience of the young Mexican botanist of 150 years ago, with considerably less fatigue and privation; but I have heard of no Latin elegiacs.

Late in the year they reached Apatzingán. On November 10 SESSÉ wrote REVILLA-GIGEDO from that place, informing him of an epidemic that had attacked all members of the party (AGH 460: 261). On November 24 he wrote again, giving thanks that all were now out of danger, although only three were free from fever; many persons had died in that part of the country. SESSÉ's own convalescence was complicated by an ulcer on his right leg, which prevented him regaining his strength by proper exercise (AGH 460: 262).

SESSÉ has something to say of the conditions under which botanical collections were made in those days and in that country. "Whoever has traveled in this kingdom [he wrote] knows very well the scarcity of food and the necessity of loading oneself with provisions if one is not to starve to death even on the best-trodden roads. Since we had to live in the wilderness, in villages of wretched Indians, in huts or tents far from habitations, we had to provide ourselves before leaving with clothing suited for the climates which we were going to encounter and with a commissary nearly equal to what is taken for a voyage at sea, or run the risk of being reduced to the scant and poor provender of the frugal and semibarbarous Indian."[31] In many of his letters mention is made of the numerous illnesses by which the members of the Expedition were afflicted, and to which CASTILLO succumbed. The modern visitor from the United States to Mexico, though able to use railroads and automobiles for part of his journeying, and to buy meals and lodging at least occasionally, still frequently falls victim to the ailments which bedeviled the explorers of 150 years ago; and can testify to

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[29] Reg. Trim. 1: 46, 346. 1832. ("una hermosa elegía en versos latinos.")

[30] The expenses of the journey were liquidated for 17,966 pesos on April 30, 1792. The list of places mentioned as visited is as follows: Querétaro, Guanajuato, Valladolid, Patzcuaro, Uruapan, Tepic, Alamos, Durango. AGH 463: VI 1.

[31] Letter of 9 May 1793; AGH 462: III 34. "Qualquiera q$^{e.}$ haya viajado p$^{r.}$ este Reyno, sabrá mui bien la escasez de viveres, y necesidad de provisiones, que es preciso cargar para no morirse de hambre aun en los caminos mas trillados. Nosotros, pues, q$^{e.}$ llamados de n$^{ro.}$ instituto, vivimos continuam$^{te.}$ en los Montes, en Pueblos de infelices Yndios, Barracas, ô tiendas de campaña distantes de Voblaciones, que puedan surtirnos de la maior parte de n$^{tros.}$ menesteres, nos vemos obligados a proveernos antes de n$^{tra.}$ salida de las ropas comodas, y proporcionadas à los climas, que nos emos propuesto investigar y de un rancho casi igual al que se embarca p$^{a.}$ los viajes de mar, so pena de estár reducidos à los poco, y viles sustentos de que se alimenta el Yndio frugal ô semibarbaro en esta parte de la Vida." A copy quoted by CARREÑO (Not. Nutka li, lii) is in AGH 527.

the misery and suffering often attendant upon the pursuit of botany in those regions.

While Mociño was thus tasting the joys and pains of his first professional collecting trip, his affairs were not prospering at home. It was at this time that his wife asked for divorce and part of his salary. In the letter to the Bishop of Oaxaca which has been already mentioned, she asserted he had been getting 2000 pesos from the Botanical Expedition, and had only given her 50 pesos in "este largo tiempo." The Bishop (in the letter of 14 August 1790, already cited) endorsed her petition and recommended her to the piety and justice of the Viceroy, saying that the unhappy lady had suffered from the genius and inconstancy of her husband and from the contempt which she had endured and concealed and even repaid with her extraordinary goodness,[32] supporting him in the growing expenses of his career. In an order dated 28 August 1790 Revilla-Gigedo directed that one-third of Mociño's salary be paid to his wife (AGH 465: XI 31). Sessé elsewhere testifies that it was impossible for Mociño to support himself, to say nothing of a wife, on what he was getting, and that he, Sessé, had charged him only half his actual living expenses (AGH 462: III 33).

Moreover Revilla-Gigedo's liberal patronage found no counterpart at the court of Spain. The "enlightened despotism" of Charles III had been succeeded in 1788 by the stupid and corrupt regime of Charles IV. The Viceroy informed the monarch, in a letter of July 1, of his provisional appointment of Mociño and the arrangement he had made for Senseve. The answer was dated, in Madrid, 22 March 1791; His Majesty demonstrated his lack of understanding of facts and values by disallowing the action of his Viceroy; Senseve, with Longinos Martínez, was ordered to rejoin the Expedition in the field at once, and Mociño, with the anatomist Maldonado who had been appointed at the same time, was to be discharged. This was received in Mexico on 4 June 1791 (AGH 462: V 38, 39).

Senseve also had profited by the situation to deliver himself of a new train of complaints, starting a file (*expediente*) which was not closed until 1793. The occasion was the stoppage of his allowance for a servant. The trouble was that three servants accompanied the Expedition in the field, each member of the scientific staff receiving a portion of their services. Since it would have been difficult to divide the servants equally, Mociño ("el Medico D. Jph. Musiños," as Senseve resentfully calls him) had fallen heir to the pharmacist's share of service without express provision for such a transfer and without any compensation being paid to Senseve. Revilla-Gigedo ultimately ordered the restitution to Senseve of 80 pesos in lieu of the lost service; a considerable hardship to Sessé, who apparently regularly paid much of the expenses of the Expedition in the field from his own pocket (AGH 462: Ia 32-51; V 2).

At some time in 1791 the Expedition, happily recovered from its ailments, reached Guadalajara, where some time was spent in arranging collections and forwarding them to the Viceroy for transmissal to Spain (AGH 462: III 33). Sessé wrote from that city on June 22 concerning the despatch of a box containing two volumes of manuscripts "en folio" and the drawings of the rarest things found on the trip (AGH 464: V 1, 2).

From Guadalajara also Mociño wrote, on June 14, the celebrated letter

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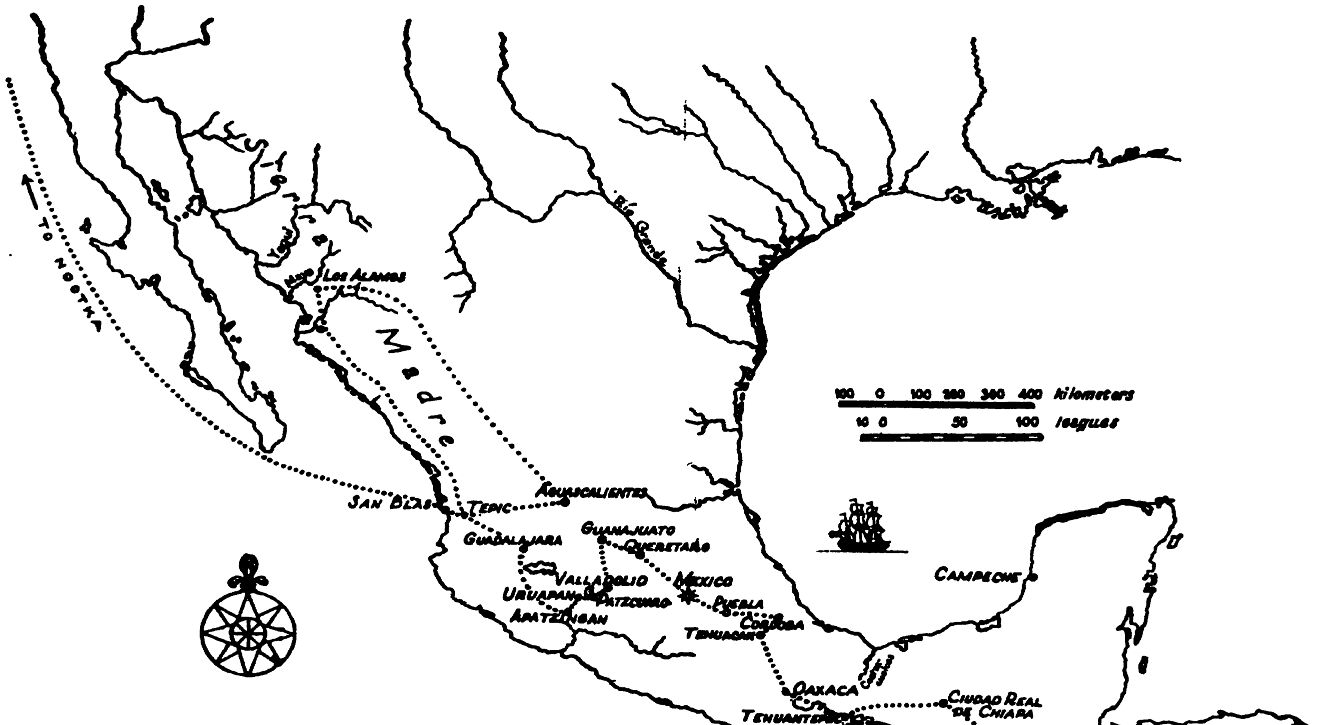
[32] ". . . desprecios de marca, q.e ha tolerado y disimulado a costa de su honrades, y prudencia, y aun se los ha pagado con finezas extraordinarias." AGH 465: XI 31.



EXPLORATIONS OF
JOSÉ MARIANO MOCIÑO
IN NEW SPAIN 1790-1793 and 1795-1799

to ALZATE on the introduction of camels into Mexico; pointing out the diversity of products to which the climate of Mexico was suited, the desirability of being independent of the old country for such products, and the usefulness of beasts of burden in protecting certain regions from famine and affording others a means of marketing their produce.

At Guadalajara the Expedition was divided. MOCIÑO with CASTILLO and one of the artists went north along the Sierra Madre "hasta los Alamos," presumably the town of that name in southeastern Sonora. MOCIÑO mentions that they explored the frigid Sierra de Tarahumar ("la frigidísima serranía de la Tarahumaria alta") of southwestern Chihuahua.[33] They crossed the range by the "Puerta de Canoas," and so came down through "Nueva Vizcaya" to Aguas Calientes, where they were to meet the others. It was on this long and arduous journey, according to MOCIÑO, that CASTILLO "lost his health." SESSÉ meanwhile went to Tepic and collected along the coast in Sinaloa as far as the Yaqui River.

While SESSÉ was at El Rosario (perhaps in Sinaloa; there are several towns of this name), he received a letter from the Viceroy directing MOCIÑO and the "best of the artists" (ECHEVERRÍA) to join the expedition which was just leaving to survey the northern boundaries of California ("la expedicion de limites de Nutka"; AGH 462: III 33). This order, dated 21 December 1791, must have been REVILLA-GIGEDO's solution of the problem posed by his Majesty's order of March 22. In this way he could keep MOCIÑO and MALDONADO employed and in the service of science, despite the royal ineptitude. CARREÑO says that his Attorney General had suggested that the discharge of MOCIÑO and MALDONADO should not become effective until the Expedition returned to Mexico (Not. Nutka xlvii). own pocket (AGH 462: Ia 32-51, V 2). SESSÉ forwarded the order to MOCIÑO in Aguas Calientes; MOCIÑO was prompt to obey, hastening to San Blas to report to the commandant of the expedition.

Of most of the details of the voyage we are unfortunately ignorant. We know that it lasted over a year; MOCIÑO says that he was away two winters. They arrived in Nootka 29 April 1792, and spent five months there (Not. Nutka 6, 67, 68). We know also that MOCIÑO mastered the native tongue ("el ydioma Nutkense") sufficiently to serve as. interpreter between the commandant and a native chief (AGH 462: III 33 *bis*). (The language and customs of these natives were of great interest to Mexican ethnologists; those who are interested may read in the pages of CARREÑO of the controversial issues involved; Not. Nutka lxxxvi-xciv). After his return, MOCIÑO prepared a report entitled *Noticias de Nutka.* Three copies of this were sent to Spain by the Viceroy; they have not been found in the Royal archives, but were said (in 1854) to be in the library of RAMON DE POSADA Y SOTO (Dicc. Univ. Hist. Geog. 5: 582). HUMBOLDT saw the manuscript at Mexico in 1803, after MOCIÑO had left (Ess. Pol. Nouv.-Esp. 2: 447, 476). A copy—perhaps the original—somehow found its way to Guatemala, where it was published in 1803 and 1804 in the *Gazeta de*

[33] Anal. Ci. Nat. 7: 214. There is a Cerro Tarahumar southwest of Los Alamos about 130 kilometers; this is probably what MOCIÑO referred to. There is also a village of Tarahumare on the Cerro Canoas farther east; this must be near the Puerta de Canoas of MOCIÑO's account. Still another place of the same name is shown by GENTRY in Bull. Torrey Club 73: 452.

Guatemala.[34] Finally a manuscript was discovered in 1912 in the library of the Sociedad Mexicana de Geografía y Estadística and was published by that body in 1913 under the editorship of CARREÑO. The work is of no botanical interest, except as it provides additional evidence of MOCIÑO's remarkable aptitude and powers of observation. In it he describes the natives of Nootka, their physical characters, their customs, government, and religion in some detail. He also reviews the controversies of the preceding few years between Spain and England, and hazards his own opinion on the merits of the case and on the future value of the island and its trade in furs. In short he is revealed as not merely an assiduous naturalist, but also a man of wide culture. His ideas were based not only on his own observations but on conversations with sailors and agents of both countries, including "el célebre navegante Wancower." Of the natural history of the island we read only that ECHEVERRÍA had made figures of 200 species of plants and of some animals (p. 67). MOCIÑO mentions the benign climate and the robust health enjoyed by members of the Expedition—a welcome change from his experiences of the preceding year.

MOCIÑO disembarked at San Blas on 2 February 1793 (AGH 462: III 33 *bis*), and presumably returned directly to Mexico. SESSÉ at once planned a new journey for the Expedition, of which he apparently continued to regard MOCIÑO as a member in spite of the Royal decree of 1791. By April 9 the Expedition was ready to set out, in two divisions. CASTILLO, MOCIÑO, and LA CERDA were to go to "la Mysteca y Costa de Tabasco"; SESSÉ with ECHEVERRÍA and the clerk JULIAN DE VILLAR to "la Guasteca y Provincia de Santander (AGH 460: 189). MOCIÑO's party left on April 20, without CASTILLO, who had fallen sick two days before; their destination now given as "la Sierra de Papalotipac y Misteca" as far as the kingdom of "Goatemala" (AGH 462: III 32, 33 *bis*.).

MOCIÑO's status was still anomalous, as noted by the Minister of the Treasury (ARANDA) in a letter of May 6, in which he asked from what funds his salary was to come, other than the "gratificaciones" (allowance for servants, etc.) already arranged (AGH 462: III 27). This provoked a determined effort by SESSÉ to have MOCIÑO, whose value and merits he conceived in the highest terms, incorporated in the Expedition. In his long letter of May 9 to the Viceroy (AGH 462: III 32-36), which has furnished much of the information about the activities of 1790-1793 detailed above, he said that MOCIÑO was now more necessary than ever to the Expedition, because of CASTILLO's serious illness, and because of the absence of LONGINOS MARTÍNEZ, who, with SENSEVE, was 200 leagues away conducting his own explorations. If, therefore, MOCIÑO were to be discharged, he, SESSÉ, would be left alone to carry on the observations of this last year (last of the six years for which the Expedition had been sent out), in which they ought to survey Misteca, the coasts of Tabasco and Tehuantepec, Huasteca, and Nuevo Santander. He notes that MOCIÑO, "este ben merito Profesor," could easily have lived quietly in the practice of medicine, but preferred the vicissitudes of travel to the farthest parts of the dominions. Few others, he wrote, could have done the work that he had done without "mil equivocaciones," and no other member of the Expedition was so well trained. To which ecomium the *Fiscal* respectfully replied that he had no authority to

[34] Not. Nutka cviii; no complete copy of the *Gazeta de Guatemala* seems to be available in the United States.

reverse the royal decision, but that they might try a new petition to the King; MOCIÑO accordingly was instructed to assemble and transmit records of his education and qualifications (Not. Nutka liv).

At this point the situation was suddenly completely altered by the death of CASTILLO on July 26.[35] MOCIÑO applied for the vacant position as soon as he got the news, on July 31, sending in the record of his education and teaching experience (AGH 464: XVI 3; XV 1-13). He was provisionally appointed by the Viceroy on 24 October 1793, at half the salary which CASTILLO had enjoyed. The royal order confirming the appointment was dated 16 September 1794 (AGH 464: XVI 7. 462: III 41); for the ensuing ten years MOCIÑO drew a regular pittance as a permanent member of the Royal Botanical Expedition of New Spain.

Of the travels of 1793 I have found scarcely any record. MOCIÑO'S destination was the Miztec country extending over parts of the modern states of Puebla and Oaxaca, and the isthmus of Tehuantepec and Chiapas—a sufficiently vast program. He would go by Puebla past the great peak of Orizaba to Córdoba, thence southwards. Of the "Sierra de Papalotipac" I can find no trace on modern maps, but the name suggests northern Oaxaca. He was in Oaxaca on May 24 when the clouds of ashes from the erupting volcano of Tuxtla reached that city (Not. Nutka 108). It was from Córdoba that he applied, on July 31, for the position left vacant by CASTILLO'S death; this must have been on the return journey. SESSÉ also seems to have been in the same town earlier in July (AGH 464: XVI 3. 460: 195. 462: "III" [IV] 1). Both he and MOCIÑO were attacked by illness; this may have curtailed MOCIÑO'S travels that year, for it is evident that he did not reach Tabasco, probably not Tehuantepec.

Later in the same year occurred MOCIÑO'S famous ascent of the active volcano of San Andrés de Tuxtla. After many years of inactivity in this isolated range by the Gulf of Mexico, violent eruptions had occurred on May 2, May 22, June 28, and August 26. The Viceroy sent MOCIÑO, by an order dated September 13, to make a report on the phenomenon, which had created a condition almost of panic in the country immediately around, being of such violence that the citizens of Veracruz, 200 miles away, had blockaded the streets under the impression that the English were attacking. MOCIÑO took ship from Veracruz, arriving in San Andrés late in September. On the 23rd he made his first ascent, with JULIAN DE VILLAR and the Alcalde Mayor Don MANUEL DE ESCOBAR.

MOCIÑO'S description of the volcano (Not. Nutka 109-112) is vivid and picturesque, as well as scientific. He speaks of the difficulty of climbing in the loose volcanic dust, particularly for one but recently recovered from a fever. They took two hours for the climb, which reduced him almost to "suffocation" (whether from shortness of breath or from the choking cloud of ash and sulfur fumes he does not say; the mountain is at present only 4500 feet above sea level); he was revived by the prompt ministration of one of the Indians with a flask of "alcali volatil." Having reached the flat top ("meseta"), they saw a spout of fire 40 yards thick roaring up from the crater, carrying a multitude of molten rocks of all sizes in a continuous series of explosions. The soil temperature was 68° Réaumur (54°C, 98°F). Two yards from the crater it was 74° (59°C), and the air more

[35] AGH 460: 197. 464: XVI 1. *See* the note on CASTILLO at the end of this Chapter.

than 70° (56°C). A stench of sulfur enveloped most of the mountain. The smoke of the great fire was of such a variety of colors that he could find no names in which to describe them; it was a spectacle so horrible that some of those who were with him ran headlong, crying that they had seen Hell itself! Mociño buried a bottle containing a paper on which was written the day and hour and the names of those who accompanied him. He collected some of the rocks which had been thrown out, remarking that their minerals could not now be identified by their forms, as mineralogists of the day contended, but needed chemical analysis.

A second ascent was made on October 21, the first clear day after a long period of storm (Not. Nutka 112-115). This time the artist Echevería went with Mociño. The volcano was relatively quiet. Mociño attempted to use various instruments which the Viceroy had sent; these had been damaged on the way and disappointed him in his hope of obtaining scientific data. Since the fire was reduced to less than a third of its former size, he had the good fortune ("tube la felicidad") to see a good way down into the crater ("aquella horribile chiminea"), which went straight down about 100 feet. The first fire seemed to have about burned out, but another vent had appeared and was burning fiercely. Less than a yard from the fire he left another bottle—one wonders why!—Villar and he, with the servant Calderon, enveloped in smoke, treading ground so hot that they could not keep both feet down on it, their legs scalded to the knees. A short distance from the crater a cold wind was blowing, and the thermometer read 14° Réaumur (20°F.).

To the natives the eruptions were evidence of divine wrath, and impelled them to abandon everything and flee their country. Fortunately these people saw in Mociño a high priest sent to assuage the fury of the volcano, or even a god capable of subduing the fire itself. They served him the more faithfully that they believed themselves invulnerable in his company. As for the bottles—they were yet another mystery or magic. He was able to restrain their panic flight by pointing out to them that the volcano was a natural phenomenon, and that in any case it was useless to hope by flight to escape divine anger. As a result of his ministrations, they stayed at home and raised a good crop of corn and beans.

I have myself struggled up through the fine volcanic dust of the cold slopes of Popocatepetl; I have seen Parícuti in eruption—from a reasonably safe distance; and I have the more admiration for the daring and fortitude of this young Mexican botanist.

Mociño apparently spent some time in San Andrés, after his second ascent of the mountain. Here he wrote out his *Noticias de Nutka,* with the *Descripción del Volcan de Tuxtla;* both are included in the manuscript referred to above, dated 27 November 1793, at San Andrés.[36] Here also he received the notice of his provisional appointment to the Botanical Expedition; on November 16 he wrote to the Viceroy expressing his gratitude (AGH 464: XVI 12).

He also wrote to his wife. Having heard from her on November 11, for the first time since September, he replied on the twenty-second. He confirmed her information about his employment, and sent her a copy of

[36] The *Descripción,* republished in 1913 with the *Noticias,* had been separately issued by the Sociedad Mexicana de Geografía y Estadística in Series II, volume 2 of its *Boletin,* and by the Sociedad Mexicana de Historia Natural in volume 7 of *La Naturaleza.*

the Viceroy's letter so that she would know just what salary he was receiving, and "would not do him the injustice of supposing that he would begrudge her whatever might be in his pocket."[37] Noting that she had been promised, but apparently had not received, one-third of his salary, he offered her half instead. It is difficult to see here the callous profligate suggested by the strictures of Bishop GREGORIO. The most that can be said to his discredit was that, being himself accustomed to poverty, he accepted his meagre fare with indifference and took no thought for the rather better living expected by his wife. It is clear from this and later letters that at this time he had no suspicion of the malice with which tongues were wagging in Oaxaca and in Mexico about his reprehesensible behavior. As for her implication that he did not live up to his promises, it appears that proper arrangements had been made for allocation of part of his salary[38]; if she did not get her third, it was the fault of His Majesty's treasury.

Early the following year we find MOCIÑO in Veracruz. On 22 February 1794 (AGH 460: 206) he wrote to inform REVILLA-GIGEDO of the despatch of a box containing collections of the preceding year made on the "Costa de Sotavento" (the "lee shore," referring to the region of San Andrés). With the box he supplied a catalogue of the contents: 30 birds, 15 bottles of "resina elastica," various volcanic lavas, a package of dried plants, and an American porcupine (AGH 460: 212). SESSÉ lists the contents as follows:[39] a few valuable plants, many exquisite birds, a sample of asfalt—"Malta, or Pisasfalto, a kind of mineral known as Chapopote, which is distilled abundantly by a mountain near Acayucán" [Vera Cruz, south of San Andrés]—and lavas from the volcano of Tuxtla. The "asfalt" or "elastic resin" was perhaps the product still known as *chapote* in Mexico, used for cleaning.

Early in 1794 MOCIÑO, with the artist ECHEVERRÍA, set out from Veracruz on the journey southwards, bound for Tehuantepec and Tabasco. SESSÉ had planned this trip on March 23 for the "preciosa estacion de Primavera" (AGH 463: VII 1, 2). He originally included SENSEVE, who at once began a querulous inquiry into the date at which the activities of the Expedition should terminate—he being afraid he would not get paid if they stayed too long. SESSÉ himself was convalescent from an illness, and LONGINOS MARTÍNEZ, as usual, busy with his own collections. On April 3 SESSÉ requested passports for MOCIÑO and ECHEVERRÍA (AGH 465: I 1). On April 12 he wrote again that "not a post fails to bring a complaint from MOCIÑO [in Veracruz] that the best season for collecting is slipping away."[40]

Meanwhile MOCIÑO seems to have been busy collecting, probably near Veracruz. On April 25 SESSÉ wrote REVILLA-GIGEDO that MOCIÑO now had ten boxes of plants in Veracruz: "the most valuable plants of that coast, such as Caobe, Gateado, Cedro fino, Balsamo del Peru, Balsamo de María, Pimienta de Tabasco, Cardamomo or Gengibre, which, not having seen

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[37] "y no me hagas la injusticia de persuadirte, qe. soi capaz de escacearte un medio, que tenga en mi bolsillo." AGH 465: XI 32.

[38] Letter from SESSÉ of April 1793. AGH 460: 191. Later, in 1803, Doña MARÍA admitted that she had had her third (AGH 465: XI 32).

[39] Letter of 3 April 1794 to REVILLA-GIGEDO. AGH 460: 207, 209.

[40] AGH 465: I 5. "No hai correo en que Dn. José Moziño deje de repetir sus clamores, al ver que se avanza la estacion mas oportuna . . ."

their flowers, he does not designate scientifically."[41] These were living plants, to be forwarded to the Royal Botanical Garden of Madrid. One wonders if they arrived and survived, and if any remains of them may still be found in the archives and museums of Spain. PABLO DE LA LLAVE tells[42] how the birds sent from Mexico and Guatemala, although received in good condition, were destroyed by moths after the boxes were opened. Of what an enormous waste of human labor, of collecting and collections, of scientific data which we would now give much to possess, were the rulers of Spain guilty!

On August 3 MOCIÑO, back in country now well known to him, wrote from San Andrés Tuxtla to the Viceroy to inform him that he was sending 22 dissected animals and birds, to be transmitted to the Director for preparation "before insects could devour them" (AGH 460: 156-159). He furnished an index to the items as follows: 1 squirrel, 1 "lanio, o paro?," 3 parrots, 1 toucan, 2 trogons, 2 martins, 3 ducks, 3 "plataleas," 3 "coarzas" [? quartzes], 1 "cirusano," 1 "ciorrion," 1 "tapa-caminos." On September 24 we hear from JUAN DE DIOS XIMENEZ in Veracruz that he has a box of dissected birds ("un caxon de pajaros disecados") for transmissal to the Viceroy (AGH 460: 213). This was presumably the same collection. From San Andrés MOCIÑO seems to have returned to Mexico—failing once more to reach the rich collecting ground farther south, but with a new hope of attaining that promised land the following year.

In 1795 MOCIÑO learned that the Expedition had been granted an extension of time, and that he was to use it in the long-projected journey to Guatemala. In April, as he was getting ready to leave, he found to his dismay that one of his companions on the journey was to be the incorrigible LONGINOS MARTÍNEZ. He sat himself down at once and wrote probably the longest letter of his career (15 April 1795, to BRANCIFORTE; AGH 464: XXIII 30-32). Having been informed [he wrote] of the necessity for a prompt start, and that he was to be accompanied by "el Naturalista Dn. JOSÉ LONGINOS MARTÍNEZ," he finds himself obliged to entreat the grace of His Excellency that he may please to release him from this misfortune; if one may speak thus of the company of a man with whom no one in the Expedition has been able to get along. Until now he had had the good fortune not to live for a moment in his company; for this reason this petition might seem unwarranted, were it not founded upon examples which he should guard against before setting out on such an extended and laborious journey through provinces where there was no ruler with the authority of His Excellency to take strong measures against the many disturbances which were sure to ensue. He wrote further of the unusual harmony which had prevailed in the Expedition while they journeyed through "Mechoacan, Nueva Galicia, Sonora, &ca.," LONGINOS being absent; a harmony which astonished even the natives. Even SENSEVE, well known as "extremadamente sufrido," had been obliged to leave him on their journey to California and to return by himself almost begging his way. Since LONGINOS was forced to rejoin the others, "I have heard, and read in

[41] AGH 460: 216. MARTÍNEZ, in his *Catálogo alfabético*, identifies some of these as follows: Caobe, *Swietenia humilis* Zucc.; Gateado, *Recchia mexicana* Sessé & Moc.; Cedro fino, *Cedrela* sp.; balsamo del Peru, *Toluifera pereirae* Baill.; pimienta de Tabasco, *Pimenta officinalis* Lindl. or *Eugenia* sp.

[42] Reg. Trim. 1: 43, 44. In 1796 a crystal flask containing various flowers in *espiritu de vino* was sent to the Garden in Madrid (AGH 463: VIII).

my correspondence, only of continued discord with the Director; a scandalous example of insubordination." MOCIÑO wrote further that his own introduction into the Expedition was "the strongest stimulus to the unjust resentment which that man, whom I have never offended, has for me personally, receiving my first visit to him with the utmost coldness and never condescending to repay it." LONGINOS would never admit that MOCIÑO was really a "profesor," would never refer to him save as a "discipulo." The young botanist's obvious favor with SESSÉ was an additional cause of hatred towards both. One can imagine MOCIÑO's consternation on learning that he was to spend months of the most exacting and isolated travel face to face with such relentless scorn and hatred!

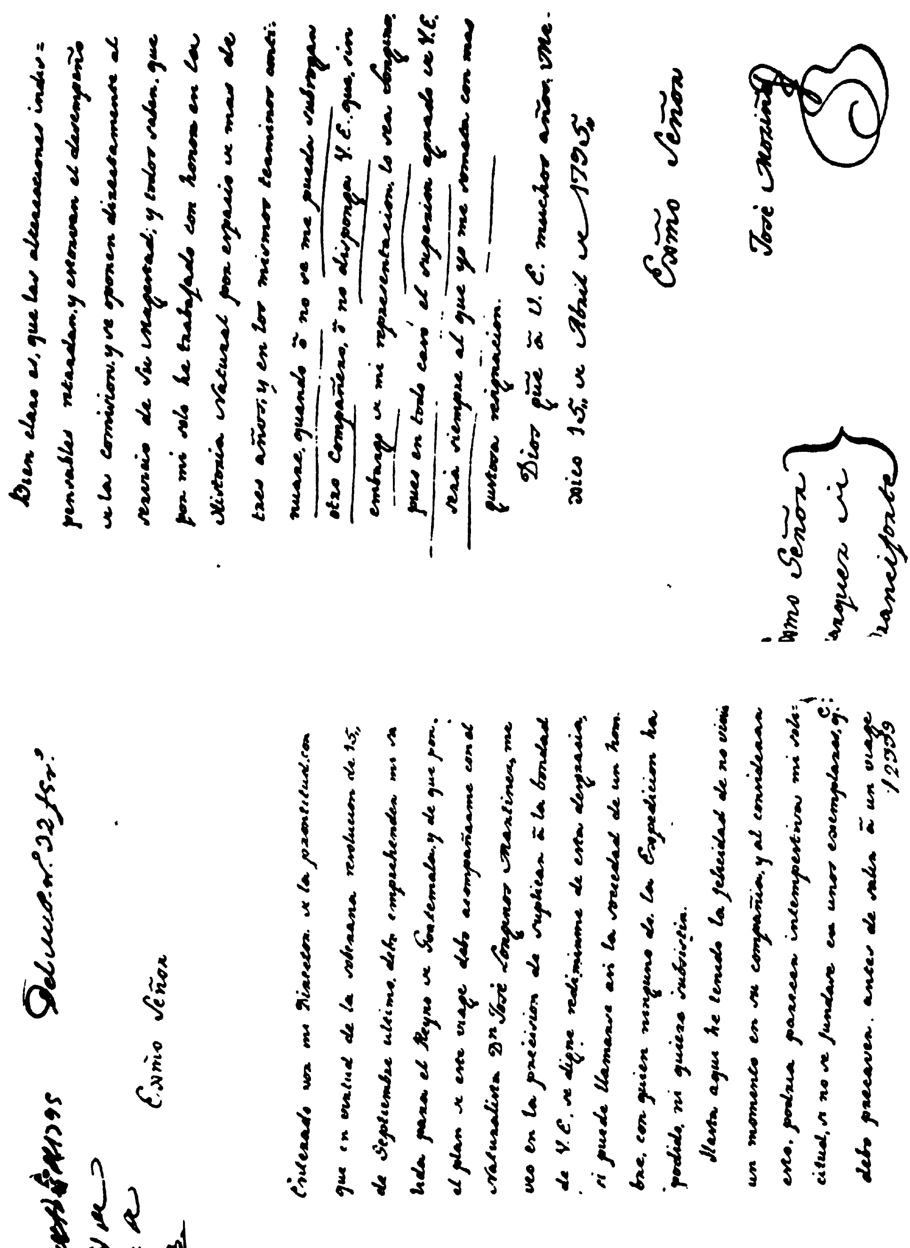
In spite of all he could say, which was reinforced by SESSÉ's own petition, the *Fiscal de lo Civil* advised strict adherence to the Royal order; LONGINOS must go with MOCIÑO.[48] The facts are reviewed in a rather typical letter of the Assessor General; the logic of rules and orders, with human values omitted, compelled a confirmation of this decision.

By this time SESSÉ was in Havana. Despairing of settling anything before spring was gone, he had left Mexico on June 22, with SENSEVE and ECHEVERRÍA (see below, Chapter III). Fortunately LONGINOS was as unwilling as MOCIÑO to join forces; he lingered in the city on one pretext or another. MOCIÑO took the only way out, asked for a passport on June 3 (AGH 465: IV 5), and left with LA CERDA on June 22—too late for spring collecting, but too early for LONGINOS (AGH 465: XII 1). Supported by CERVANTES, he made an effort in June to have JULIAN DE VILLAR added to the party for Guatemala (AGH 465: VII 4-6). A petition was addressed to His Majesty, with the approval of BRANCIFORTE; it was refused on November 22—VILLAR was not a "Profesor" (as we should say, he had no degree). By this time MOCIÑO and LA CERDA were far from Mexico; the royal decision caught up with them nearly a year later, in Guatemala. It appears that for a time they had planned to support VILLAR from their own slender resources (AGH 465: VII 1-10), though it is not clear whether or not he accompanied them on part of their journey.

Of the movements of MOCIÑO and his companion on this journey we have a detailed record in the *Archivo General.* The compilation and preservation of this record were occasioned, characteristically, by the failure of the colonial government to pay the explorers after their return to Mexico. During the journey they had collected their salaries from the provincial cities through which they passed; the receipts which they gave were later forwarded to the capital for collection, and were assembled in a file in order to establish the period for which pay was still due (AGH 465: VI 8-26; XII 4-11).

On 22 June 1795, accordingly, MOCIÑO and LA CERDA took the now familiar trail to the south. They seem to have traveled slowly, doubtless collecting as they went; and they were now entering the rainy season, which rendered travel more difficult even than usual. On July 29 they were only as far away as Puebla. From this city MOCIÑO wrote back to BRANCIFORTE asking for a tent ("una de las tiendas de campaña") to protect not only their persons but their books and collections from the rain; since they must pass through country destitute of habitations. BRANCIFORTE, however, re-

[48] Letters of 18 May 1795 and 8 June 1795. AGH 464: XXIII 77-83. For the details of this business see below, Chapter IV.



Junio 15 ... 1795
... ... de
... Hacienda: a
... ...

Del ... n.° 32 fs. ...

Exmo Señor

Enterado mi Director de la prontitud con que en virtud de la soberana resolucion de 15, de Septiembre ultimo, debo emprehender mi salida para el Reyno de Goatemala, y de que por el plan de este viage debo acompañarme con el Naturalista Dn José Longinos Martinez, me veo en la precision de suplicar á la bondad de V. E. se digne redimirme de esta desgracia, si puede llamarse asi la sociedad de un hombre, con quien ninguno de la Expedicion ha podido, ni quiere subsistir.

Hasta aqui he tenido la felicidad de no vivir un momento en su compañia, y al considerar esto, podria parecer intempestiva mi solicitud, si no se fundare en unos exemplares, q. debo precaver, antes de salir á un viage

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Bien claro es, que las alteraciones indispensables retardan, y estorvan el desempeño de la comision, y se oponen directamente al servicio de Su Magestad; y todos saben, que por mi solo he trabajado con honor en la Historia Natural por espacio de mas de tres años, y en los mismos terminos continuare, quando ó no se me pueda subrogar otro Compañero, ó no disponga V. E. que, sin embargo de mi representacion, lo sea Longinos, pues en todo caso el superior agrado de V. E. será siempre al que yo me someta con mas gustosa resignacion.

Dios gũe á V. E. muchos años. Mexico 15, de Abril de 1795.

Exmo Señor

José Moziño

...mo Señor
...anquez de
...ranciforte

Parts of the letter from Mociño to Branciforte (15 April 1795) in which he protests at having to accompany Longinos in his journey to Guatemala. He begins as follows:

"My Director having gone into the promptitude with which, in virtue of the sovereign resolution of September 15 last, my departure for the Kingdom of Goatemala should be undertaken, and that in the plan of this journey I am to be accompanied by the Naturalist Don José Longinos Martínez, I find myself obliged to appeal to the goodness of Your Excellency that you may see fit to spare me this misfortune, if I may so describe the society of a man with whom no one of the Expedition has been able to get along, nor wishes to.

Until now I have had the good fortune to have lived not a moment in his company, and when this is considered my solicitation may appear exaggerated, if it were not founded on examples which I ought to guard against before leaving on a journey so extended and exacting in Provinces in which there is no officer with the authority of Your Excellency. . . ."

fused this request, apparently having other uses for tents (AGH 465: IV 6, 7). The travelers had to find what shelter they could in the desolate mountains of Puebla and Oaxaca; they seem not to have gone far from Puebla during the rainy season.

On September 6 and 7 they were Tehuacán; whence, apparently, they returned to Puebla on September 23. By November 7 they were in Oaxaca, where they lingered a month or more. It was at this time that Mociño saw his wife, renewed his offer to her of half his salary, and promised her a written contract (AGH 465: XI 32).

Early in 1796 they were on the move again. They left the great valley of Oaxaca, crossed the high mountains to the east (passing through country still wild, still insufficiently known to botanists), and descended into the valley of the Río Tehuantepec. On January 20 they drew their pay in Zoquitlán; in February they were in Tehuantepec. In this part of their travels they could enjoy, perhaps for the first time in their experience, the wealth of tropical lowland forest. They could explore the country by dug-out canoe or by jungle trail, hearing the monkeys howl in the tall trees, glimpsing the little parrots flashing through the tangle of vines and branches, visiting the thatched huts of Indian villages that had been little affected by the coming of white man (and, indeed, are little changed today). We have no record of the emotions with which they explored this botanical paradise, nor of the fruits of their collecting. We can but infer their activity from the length of time during which Tehuantepec was their headquarters. Their receipts here are dated February 9, March 26, and May 14; a mellow season of dry weather and mild sunshine.

On June 6 Mociño wrote to Branciforte from "Ciudad Real" that he was sending a box of specimens for Cervantes. With the Ciudad Real de Chiapa (now known simply as Chiapa or Chiapa de Corza) he was to become better acquainted on the return journey. Evidently the trail lay through the mountains to Guatemala. A long gap in the record here may perhaps be interpreted as a corresponding period of collecting in new territory. By December 1 they were in the capital city of "Nueba Goatemala."

They apparently pressed on, without much time for local exploration here. They were in San Salvador on 4 March 1797. Violent earthquakes are said to have occurred in this city while they were there, Mociño's "little fortune," his personal possessions, being buried in the ruins.[44] On May 31 they were in León de Nicaragua. This city marks the southern limit of their official journey. With it as headquarters, however, they must have made a fairly thorough survey of the surrounding country; for they stayed there nearly a year (other receipts are dated October 2 and December 2). We must admire the devotion to science which impelled Mociño to hold to his purpose regardless of time spent; for the two extra years were now gone, and he was far from home. We know what Senseve would have said, and done, under like circumstances!

Early in 1798 they began the long journey back towards Mexico—home and civilization. On February 2 San Salvador saw them again, on May 9 Guatemala City. Here Mociño is said to have analyzed the drinking water, and to have studied three of the chief products of the country,

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[44] Dicc. Univ. Hist. Geog. 5: 582. Not much credence should be given such a story, however, from this often unreliable source.

sulfur, mercury, and indigo.[45] August 29 found them in Chiapa; here the journey was interrupted, for MOCIÑO the physician, by an outbreak of leprosy which had apparently become serious. Whether he was drafted by the local authorities, or whether the plague aroused his always ready scientific and humanitarian impulses, it is certain that he was granted permission by the new Viceroy, Don MIGUEL JOSÉ DE AZANZA, to stay in Chiapa and put his medical skill at the service of that community (AGH 465: XII 2. Not. Nutka lxxi). MOCIÑO did not arrive in Mexico until 3 February 1799. His fellow-traveler had reached the capital December 24.

MOCIÑO and LA CERDA must have harbored, at this time, some bitter thoughts on the gratitude of kings. On 23 October 1799, they informed the Viceroy that they had received no pay since they left Guatemala, and had no other means of support; yet they had continued to fulfill their duties in the expedition, though they could have earned a living otherwise (AGH 465: XII 12). On November 9 they were still trying to pierce the formidable barriers of bureaucratic red tape that made payment of their salaries so difficult; officially, doubtless, they were still in Guatemala. They wrote that they had had to sell or pawn even their most necessary possessions, had nothing to eat, and, finally, were "daily put to shame by the collectors for the rents of their houses, by the tailors for the mending of their poor clothing, and, in a word, by their very bellies, which would accept no terms." A note at the end points out that they were unable even to afford a properly sealed letter.[46] The costs of the journey were finally liquidated on 27 January 1801; let us hope that they were paid at that time (AGH 465: XII 18).

With his return to Mexico in 1799, MOCIÑO seems to have ended active collection. SESSÉ had come back from Havana the preceding year, and the Expedition was now reunited, save for the independent and scornful one. There ensued a period of "arrangement of collections," of sorting, classifying, and naming of specimens. MOCIÑO resumed his interest in medicine, and spent many hours in the hospital of San Andrés and in other hospitals making careful observations of the effects and virtues of medicinal herbs, many of which he had himself collected. He is said to have made from four to six visits a day to the hospitals over a period of two years (Dicc. Univ. Hist. Geog. 5: 582. Contr. U. S. Nat. Herb. 23: 15).

MOCIÑO's revived interest in medicine found expression in the work of editing and enlarging JOHN BROWN's *Elements of medicine,* which was printed in a Spanish version in Mexico in 1803. During this period several small articles by MOCIÑO appeared in the *Anales de ciencias naturales.* The first of these was an extract from a lecture given at the opening of the class in botany in 1801; doubtless a glad occasion for the veteran teacher, CERVANTES, when he could thus honor his most outstand-

[45] Dicc. Univ. Hist. Geog. 5: 582, 583. The author of this account says also that MOCIÑO wrote on indigo; I have found no publication on this subject, but it is mentioned in a letter from MOCIÑO of 21 March 1803 (AGH 465: unnumb. file).

[46] AGH 465: XII 14. "En fin, no tenemos q^e· comer: diariamente nos sonrojan los cobradores p^a· el alquiler de nuestras casas, los sastres p^a· el remiendar de nuestre pobre ropa, y en una palabra nuestro vientre mismo q^e· no admite plazos. Nota. No và en papel sellado p^r· q^e· no hay con q^e· comprarlo."

THE GARDEN OF THE PALACIO NACIONAL: the last relic of the Royal Botanical Garden of 1788-1829. This view looks westward towards the front of the Palacio and the Zocalo In CERVANTES' time the wall on the right hand had not been built; the garden was much larger.

ANOTHER VIEW OF THE GARDEN OF THE PALACIO NACIONAL, looking towards the east. The fountain in the background may conceivably occupy the site of the "tanque" for which CERVANTES fought so long. The buildings on the left are of more recent date.

ing pupil.[47] MOCIÑO's subject was plant drugs and remedies. He spoke with some asperity of the lack of a *Materia medica* proper to the country and of the existence in the *boticas* only of drugs and simples of European origin, often adulterated. He discoursed on many medically useful plants of New Spain, and said that the Botanical Expedition had a long catalog of them, the virtues of which it was his purpose to test by actual observation. Two years later appeared an article on *Polygala mexicana,* a complete description of the plant and its medical virtues (An. Ci. Nat. *7*: 48-54. Naturaleza *7*: apend. 43-46). This was followed by an article on "la resina del Ule," the latex of the plant which CERVANTES had named *Castilla elastica* (An. Ci. Nat. *7*: 212-215. Naturaleza *7*: apend. 46, 47). A writer in the *Gazeta de Bayona* had confused this with the "resina elástica" or "chapopote" of volcanic origin, mentioned above. MOCIÑO here resolves the confusion.

NOTE ON CASTILLO

Don JUAN DE CASTILLO, to whose death MOCIÑO owed his appointment as a permanent member of the Expedition, was born in Jaca, Aragon, in 1744.[48] His parents were Don DOMINGO DE EL CASTILLO and Doña JOSEFA LOPEZ VEZINOS. He studied Latin and pharmacy, and at the age of 27 went to Puerto Rico as *boticario mayor* of the Royal Hospital, where he stayed 17 years. This brings us to 1788, when he went to Mexico to join the Botanical Expedition. He seems to have been unfortunate from the beginning, having suffered more than his share of the sicknesses which afflicted them all on their collecting trips. His fatal illness was diagnosed by SESSÉ as having "sintomas de escorbuto" (AGH 462: III 32), but CERVANTES described it as "una obstruccion en el píloro."

A copy of his will is preserved in the Archivo General (AGH 464: XVI 18-22). After attesting, in the approved manner, his belief in the Divinities and other mysteries, articles, and sacraments of Holy Mother Church, in which as a good and loyal Catholic he had lived and proposed now to die, and having besought the intercession of the patron saints of Aragon, of Jaca, of his own name, and of all the angels and saints of the Celestial Court, and being now in fear of death, as is natural to all living creatures, and its hour uncertain, he bequeathed and made and ordered his last will and testament. A sum of 4000 pesos was left for the printing and engraving of the *Flora mexicana,* that great work to the accomplishment of which all the members of the Expedition looked forward as the crown of their labors, for which he had worked, CERVANTES said, "con esmero," and which, as SESSÉ said, had shortened his life (AGH 464: XVI 1, 2).

SESSÉ, informing the Viceroy of CASTILLO's death, asked him to have a notice of the event inserted in the *Gazeta,* while the Expedition considered what could best commemorate their fallen comrade. They decided (AGH 464: XVI 14) that the most glorious memorial (*obsequio*) to perpetuate his name would be to bestow it upon one of the new plants which they had discovered, using a portion of his estate to print and illustrate this con-

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[47] An. Ci. Nat. 5: 288-296. N 1802. Naturaleza 7: apend. 39-42. 1885. This is cited also by BERISTAIN (Bibliot. Hisp.-Am. 2: 353) as "Discurso sobre la materia médica," and assigned the date 1801; it probably appeared in pamphlet form in that year.

[48] These facts are taken from the brief biographical note by CERVANTES in his *Discurso* of 2 June 1794, as reprinted in La Naturaleza 7: apend. 18; and from the preamble to CASTILLO's will, cited below.

tribution to knowledge and to fame. From this came the naming of *Castilla elastica* by CERVANTES.

In 1791 efforts had been inaugurated towards a subscription for the printing of the *Flora*. A printed circular was issued on September 17, referring to the expeditions to Peru and Nueva Granada, and calling attention to the loss of HERNÁNDEZ' drawings in the burning of the Escorial Library in the preceding century. Subscriptions were invited from the clergy, from civil officials, from professors, and so forth. Regret was expressed that the royal treasury was unable to support the costs of publication (AGH 464: VIII 2, 3). By 28 February 1793, 106 pesos had been received from nine *padres curas* of Oaxaca, and the archbishop, ALONZO NUÑEZ DE HARO, offered 2000 pesos (AGH 464: VIII 9, 13-16). A new circular was sent out in 1793 to the bishops of Puebla, Valladolid, and other dioceses, to the deans and *cavildos* of Yucatan, Durango, and other provinces, to the municipalities (*ayuntamientos*) of Zacatecas and other places—in all to 40 addresses. In the list preserved in the archives nine of these are marked with a cross, including the dean and *cavildo* of Oaxaca, indicating presumably that these had contributed (AGH 464: VIII 25). Other small contributions dribbled in from other places: 434 pesos from the Presidio de Nuestra Señora del Carmen, 29 pesos 4 reales from Pueblo de San Joaquín de la Palizada, in amounts from 4 reales (half a dollar) to 150 pesos (AGH 464: XI 4-6). By 1808 the total amounted to 9377 pesos 4 reales—including CASTILLO's 4000 (AGH 464: VIII 32). In that year a number of subscribers complained that they had heard nothing further—Guadalajara, Salvatierra, Villa de Carrion del Valle de Atlixco, the Santa Yglesia Metropolitana de México, Orizaba, and others (AGH 465: various letters, not gathered in a file). It would be interesting to know what finally became of these funds. CASTILLO in his will had stipulated that if the money were not used for the stated purpose within six years, it was to be applied to founding a *Real Posito de Granos* in his birthplace.

Chapter III

SESSÉ: THE WEST INDIES

Towards the end of the six years allotted to the Expedition, SESSÉ must have been dismayed to realize that the vast floral wealth of New Spain had scarcely been sampled. His time would be up in June 1794. Moreover sickness and death, the absence of LONGINOS MARTÍNEZ, and MOCIÑO's voyage to Nootka had [he wrote] interfered with the organization of the notes made in the field and the completion of the sketches of rare and unknown species. Yet they had covered more than 3500 leagues (without counting the voyage to Nootka), and he could say that the materials in their possession would form a Flora as rich as that of any other kingdom; and that though animals were not especially abundant in Mexico, many new species, particularly of birds, would be recognized. The work would be much more complete if they could survey the very fertile Kingdom of Guatemala and the islands of Cuba, Santo Domingo, and Puerto Rico, where "abundan los Balsamos mas exquisitos, y otros producciones de mucho interes en el Comercio y Medicina." The death of CASTILLO and the separation of MOCIÑO had prevented the exploration of Guatemala which had been planned for 1793, and he proposed to survey this realm, "que seguramente es la mas fertil de la dominacion Mexicana," during the current spring.[49] This rehearsal of the troubles and delays of the Expedition was the prelude to a petition that His Majesty might be pleased to grant an extension of two years in the time allotted to their survey.

The petition was granted, on September 15. Not until 15 February 1795 were copies of the royal order made, and SESSÉ was notified on April 6 that two additional years might be expended, providing that his new project be commenced in the spring of 1795, "sin admitir V. E. pretextos ni disculpas que demoren la salida ni el tiempo des los dos años que pretende el Director," and prescribing strict economy (AGH 461: VII 10, 464; XXI 6, 8). More than a year had elapsed since he wrote his original petition, and the spring which he was required to use was already gone. He made, however, all speed to get his Expedition under way. He made a fruitless effort to get extra servants—two for each portion of the divided Expedition, instead of three for the whole; he came out one *mozo* short. He requested and got free passage to Havana, this being counted as the first leg of the journey back to Spain; he had to give up all claim to double pay for the time of the voyage (AGH 464: XXIII 1-3). He got MOCIÑO and LA CERDA off for Guatemala, as has been told elsewhere. He, with JAYME SENSEVE and ATANASIO ECHEVERRÍA, left the capital for Veracruz on 22 April 1795 (AGH 461: VII 4, 17, 49). He attempted to dispose of the LONGINOS MARTÍNEZ problem en route—at Puebla on April 24. He sailed for Havana on May 5 and arrived there on May 30 (AGH 464: XXIII 75, 96. 465: III 4).

SESSÉ's administrative troubles disembarked with him in Havana. SENSEVE had been brought away in the midst of one of his eternal disputes

[49] The same letter was sent to REVILLA-GIGEDO on 29 March 1794 and to Don EUGENIO DE LLAGUNO AMIROLA on 26 April 1794. AGH 460: 230-232. 464: XXI 1.

about pay and allowances; the viceregal disposition of this was acknowledged by SESSÉ on June 8, shortly after they had landed (AGH 462; V 15-17). SESSÉ himself ran into difficulties with salaries; the *Yntendente* of Havana refusing, for some technicality or other, to disburse (AGH 465: III 9). This was on July 10. And ECHEVERRÍA was already down with dysentery. By August 30 the salary question was settled; but the artist was not only still too ill to travel—he could not even work at the sketches made in Nootka, which he had promised to complete (AGH 465: III 16). SENSEVE also was ill with the same complaint. The rainy season having arrived, "la furia de las aguas" was added to their other miseries, making it almost impossible to go out until the end of September (AGH 461: VII 50-54). "Considering the labors [he wrote in 1798] which we suffered on those swampy shores, impassable in time of rain, with the disappointment of not obtaining a reward commensurate with the effort, since only rarely did a plant keep its flowers until this season and consequently few could be studied with exactitude, we may almost say that this year was lost as far as our subject was concerned." However, he did succeed in classifying and describing many of the "rare fish" which swarm in these seas, completing the work of ANTONIO PORRA. Early in November they came back to Havana, where they spent the winter in the arrangement of what they had managed to collect and in dissecting and sketching the fish.

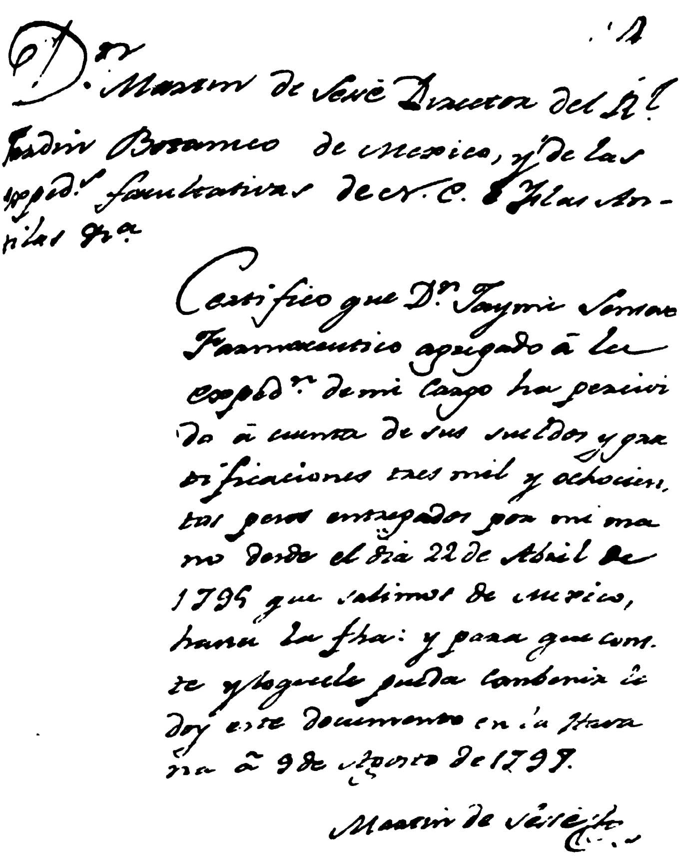
Early in 1796 they sailed for Puerto Rico, leaving Havana on the frigate Gloria about March 2 and landing in Puerto Rico March 28 (AGH 465: III 17. 461: VII 50-54). On April 9 they began their observations in "aquella amenisima Ysla," which they continued until the end of September. The smooth tenor of their labors on this island may be inferred from the complete lack of notice of them in the archives. Even SENSEVE must have been happy.

At this point the progress of botanical exploration was interrupted by war. The English attacked Puerto Rico and blockaded the islands. The botanists were at some pains later (in trying to collect their salaries) to explain their departure from their projected schedule. SENSEVE wrote on 27 November 1798 that "without doubt they could have finished their work in the two years granted by the King; but everyone knows of the invasions and assault made on that place [Puerto Rico] by our enemies, blockading it in a fashion which I need not describe" (AGH 461: VII 17-22). For a time they had hopes of embarking for Havana in a Catalan brigantine. Various Americans refused to take them aboard, since their presence would betray to the English that the vessel had been in an enemy port. Finally they enlisted the governor to prevail on Mr. FLORENCE DRISCOLL, Captain of the American ship McGilvra, to carry them across for 200 dollars, not including their board.[50] They were to sail 20 April 1797, but the blockade became effective on April 17, and departure was delayed until May 12. They reached Havana June 1, having narrowly escaped capture several times, according to SENSEVE (AGH 461: VII 17-22; 50-54).

The record contains the complete list of provisions purchased for seven persons on this 19-day journey; apparently, however, this was designed also to include the 14-day voyage from Havana to Veracruz (AGH 461: VII 76). This *rancho* included 6 calves with feed for them, 3 dozen hens

[50] Receipt given by Captn. DRISCOLL of American ship McGilvra to "Dn. Martin Sesse Derector of the Expedison of the Botinest Company in the Windward Islands from the Cort of Spain." Havana 30 June 1797. AGH 461: VII 77.

and 2 dozen chickens, with corn for them to eat, 4 dozen pork sausage (*chorizas*), 16 yards of *salchicho* (another sausage), 25 lb. of rice, the same of sugar and of onions, 12 lb. of coffee, chick peas, beans, eggs, almonds, raisins, chocolate, a barrel of biscuit (*galleta fina*), 6 loaves of bread, 2 *racimos* of bananas, butter, sundry jugs of oil and vinegar, 6 bottles of brandy, one barrel of red wine, and one English cheese; all at a cost of


D.n Martin de Sessé Director del R.l
Jardin Botanico de Mexico, y de las
expedic.s facultativas de N. C. en Yslas An-
tilas &a.

Certifico que D.n Jayme Senseve
Farmaceutico agregado a la
expedic.n de mi cargo ha percivi
do a cuenta de sus sueldos y gra
tificaciones tres mil y ochocien,
tos pesos entregados por mi ma
no desde el dia 22 de Abril de
1795 que salimos de Mexico,
hasta la fha: y para que cons
te y lo que le pueda convenir le
doy este documento en la Hava
na a 9 de Agosto de 1797.

Martin de Sessé

Certificate by Sessé of payments to Senseve on the expedition to the West Indies. It runs as follows:

"I certify that Don Jayme Senseve, Pharmacist attached to the Expedition of which I have charge, has received on account of his pay and perquisites 3080 pesos transmitted by my hand since April 22, 1795, when we left Mexico, until the present; and that this may be established I give him this document in Havana, 9 August 1797."

some 300 pesos. The lack of maize is striking; evidently *arroz con pollo* was then, as now, one of the staple articles of diet on a journey in New Spain.

As soon as they landed in Havana, Sessé announced his intention of exploring the western part of the island, in which he had been prevented by the misfortunes of 1795. To the ever-querulous Senseve we are indebted for much of the remaining history of this adventure; for he characteristically objected that the two years allowed had passed, and that they would never be paid for any further work. With this begins the bulky *expediente*

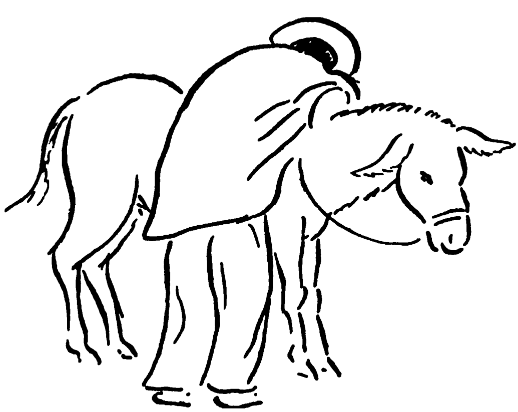
in the archives entitled "Sobre abono de sueldos pr. las caxas grales. de Dn. Jaime Senseve" (AGH 461: VII).

In several letters SESSÉ told SENSEVE that he could go back to Mexico alone if he must (and doubtless added in his thoughts another place he could go to); for himself he was going to seize the opportunity to collect in Cuba, and would be busy with his collections and manuscripts at least until the other party returned from Guatemala. On August 9 SENSEVE requested passage and maintenance for himself and servant; this was granted, and he sailed September 11 in the brigantine San Carlos, commanded by Captain TOMAS DEL CALVO; landing in Veracruz September 25. A passport was issued on September 27 for the journey overland to the capital, where he arrived, "algo enfermo," on October 5; and immediately fell again to letter-writing. Here the correspondence rapidly loses itself in a labyrinth of technical detail, the *expediente* traveling from one office to another and ever acquiring new letters, orders, endorsements, until the brain reels. The principal trouble was that certificates had been issued for payment of SESSÉ and SENSEVE together, and it baffled the bureaucratic brain to pay SENSEVE by himself. Furthermore SENSEVE wanted *double* pay for field duty; whereas the *Fiscal* wanted to pay him only *half* his base salary because the time allotted to the expedition had run out, and they were officially back in Spain on half pay! This good official had evidently thought, when they left for Havana in 1795, that he had seen the last of them. Moreover, since they had traveled at government expense, there was a question of the date from which salaries were to be reckoned. In this nightmare of official obfuscation SESSÉ's long letter of August 1798, already often quoted for the details of the entire journey (AGH 461: VII 50-54), sheds a welcome light of clarity and reason. "Three difficulties [he wrote] have been brought out by the scrupulous zeal of the *Sr. Fiscal de Real Hacienda* and by the very exact scrutiny of the *Ministros de estas caxas* . . . ." and he goes on to explain all the reasons for his delays and postponements. The account was finally liquidated 20 December 1798, after SESSÉ himself was back in Mexico. He had been ordered in a letter of 29 September 1797 to return to Mexico at once (AGH 464: XXIII 98, 99). He replied by giving an extended account of the part played by the British navy in his affairs. On November 18 he wrote that "we know nothing here of Spain because the Americans, whose are the only boats which are reaching this port, never speak words of truth, since it does not suit their business" (AGH 465: IXb 1, 2). He finally sailed 18 March 1798, arriving in the capital on May 12 (AGH 461: VII 54).

SESSÉ was unfortunate in his colleagues. On November 18 he wrote BRANCIFORTE that, though he was prepared to risk enemy action and the seasonal tempests of the Gulf of Mexico, he was embarrassed to lose the painter ECHEVERRÍA, who had deserted to the expedition of the Count of TARUCO.[51] SESSÉ had protested the incomplete state of his work, but ECHEVERRÍA "shut his ears to my comments and his eyes to the state of his sketches." Since the two years were up he was technically free; he succumbed to the temptation of 1000 pesos a year (he had been getting 500), with expenses paid. This was a serious loss; an artist of his talents was irreplaceable.

[51] In AGH 465: IXb 3 this name appears to be written "Ponpos y Taxuco." Elsewhere it appears as "Conde de Popox." *cf.* 465: XIV 3.

Long after the whole Expedition was reunited in the capital, an aftermath of the West Indian adventure troubled both scientists and officials. When the ministers of the Royal Treasury went over his accounts, they objected (17 January 1801) that the cost of the *rancho* for the return from Puerto Rico was grossly inflated (AGH 461: VII 81). To this SESSÉ replied, with his usual vigor and lucidity, that he assumed His Excellency did not imply that he had falsified the account, but was referring only to the quantities purchased; and that having in mind the dangers and uncertainties of travel by sea, it was common practice to lay in supplies for a longer time than was actually anticipated. Evidently, he went on, the accountants in Mexico did not realize the high cost of living in Havana—onions, for example, at more than a dollar a pound (AGH 461: VII 83-86). It was finally recommended (18 March 1801) that SESSÉ's claims be satisfied, since he formally declared that he had disbursed the amount in question; but he was censured for lack of the prescribed economy, seeing he paid as much as 3 pesos for two bunches of bananas—"when it is well known how abundant and cheap this fruit is in Puerto Rico." The distinct flavor of modernity here evident is accentuated by the discovery that all members of the Expedition regularly paid a "war tax" of 4 per cent, which was deducted from their salaries.



Chapter IV

LONGINOS MARTÍNEZ: MUTINY IN THE EXPEDITION

Institutions of learning are unfortunately not exempt from petty jealousies and internecine strife. The Botanical Expedition was plagued from the beginning by friction between irreconcilable personalities. In the midst of the bickering we may usually discern the tragic figure of José Longinos Martínez, *naturalista.*[52]

This man's title signified about what "biologist" does today; both being improperly used to designate one interested in the study of animals rather than plants. It is understandable that these two "ramos" might not always prosper in the same places; country rich in plants is not always the best for the collection of animals. Consequently Sessé's policy of keeping the entire group together in their journeys gave Longinos' naturally rebellious and independent nature a plausible pretext for resentment. At all events, we find that even in 1788 he and Echeverría had detached themselves from the party—this time apparently with Sessé's assent—remaining in Mexicalzingo to make dissections and drawings until December 7, after the rest had returned to Mexico (AGH 460: III 136). By 1790 mutual distrust had developed to the point where Longinos refused to accompany the Expedition when it set out for Michoacán and Sonora on May 17, but "por considerarse independiente" stayed in Mexico, ignoring Sessé's repeated inquiries (AGH 462: V 38, 39). We are indebted to the recalcitrant for much of our knowledge of Sessé's route, since the latter wrote on this matter to the Viceroy from points along the way (AGH 460: 271, 273). Revilla-Gigedo had meanwhile (May 21) requested an explanation from Longinos; who pleaded sickness and apologized for his delay also by explaining that he wanted time for the mature reflection necessary to answer the charges of the Director (AGH 460: 265, 275).

It is probably to this period that we may assign a long letter in which Longinos poured out all his grievances against Sessé and sought to justify himself in the eyes of the Viceroy.[53] He had [he wrote] kept deep silence on various matters pertaining to the service of the King, thinking that the Director might change his views, and being unwilling to disturb the harmony of the Expedition; but seeing that the Director had made unfounded complaints about him to His Excellency, it was opportune that he lay the truth before him. The Director [he continued] was chosen administrative head, as knowing the Mexican language, and so that he might propose measures for making the Botanical Garden self-supporting; but he neither possessed that tongue nor had he made such proposals. Sessé himself

[52] According to Beristain (Bibliot. Hisp.-Am. 2: 198), he was a native of "Logrono en la Rioja," on the upper Ebro in northern Spain, and had been chosen by Gomez Ortega himself for the Expedition.

In referring to him below as "Longinos," I follow the general practice of his contemporaries.

[53] Letter of 25 May, year illegible, no signature, no address. AGH 464 X 1-8. The presence of the Expedition in Cuernavaca in 1789 is confirmed by a letter from Sessé dated there December 12 (AGH 460: 277).

[he went on] confessed that he lacked science, "both natural and botanical"; so, although he was Director, it was not as being a scholar. His authority should therefore be limited to administrative matters. His Majesty had plainly commissioned him, LONGINOS MARTÍNEZ, as naturalist, and in chapter 4 of his title made him expressly responsible for all things pertaining to his *ramo,* including the formulation of his results when he had returned to Spain. Moreover, all matters needing deliberation must, by an article in the regulations, be put to a vote; this SESSÉ had never done, but enforced his own will in matters affecting sciences which he did not possess. His amplification of this point gives us a clue to the movements of the Expedition. As a consequence of SESSÉ's insistence that the Expedition travel and work as a group, it had spent "in the preceding year five months in Mazatlán, where the naturalist had to fill in his time with botany, not having any occupation of his own in that place." This must be Mazatlán in Guerrero. Further on he mentions Cuernavaca, "where the Expedition remained a little more than a month," and where the naturalist apparently had too much to do instead of too little; and Acapulco, where he got through three months' work in 10 or 12 days by working through the siesta and at night and employing *mozos* and fishermen, because the Director insisted on the presence of everyone in Cuernavaca by a certain date. The Expedition had evidently traveled the road so well worn since then by all the tourists; this must have been in 1789, subsequent years being otherwise accounted for. He complained further that SESSÉ kept all the books and supplies to himself in his own house, to which he required the others to come "like schoolboys," and made them submit to him all their collections to enhance his own glory. The naturalist had formed a *gabinete* at his own expense and in his own house, with proper cases; to this the Director was always opposed and did his best to prevent collections from being placed in it—even imperfect articles rejected by the Expedition.[54] Collections sent to the Director's house, "en fuerze de imperiosos mandatos," were ruined by neglect or by mishandling, when "the Director, who when the naturalist arrived in the kingdom had never seen a dissected animal in his hand, nor had any notion of this science, changed himself into a Master of Natural History and attempted to correct half of these animals. Objects of natural history need such delicate handling that an inexperienced person can destroy their value even by taking them in his hand . . . . Ningun Profesor de honor [he wound up] pueda seguir baxo de semejante sistema," and LONGINOS petitioned His Excellency that the naturalist be authorized to conduct his own researches independently, keeping his collections in his house, boxing them for Spain, and merely notifying the Director of his progress.

It is easy to find, in this tirade, elements of truth. It is entirely probable that SESSÉ had made mistakes both in the direction of the Expedition and in the identification of specimens. But the picture as a whole is obviously distorted by the thwarted ambitions of an angry and intolerant man, whose tone varies from bitter sarcasm to fawning subservience, and whose facts are everywhere mixed with fiction. He was ultimately on bad terms not

[54] This small private museum was well known in Mexico, and must have had considerable merit. BERISTAIN (Bibliot. Hisp.-Am. 2: 199) describes it, somewhat redundantly, as "una copiosa tanto como preciosa colección de minerales, vegetales y animales de los tres reynos de la naturaleza, de que remitió muchos cajones al Gabinete de Madrid."

only with SESSÉ but with all his colleagues, including the sprightly MOCIÑO and the inoffensive SENSEVE.

SENSEVE also was left behind when the Expedition set out in 1790, in the arrangement by which MOCIÑO joined it. Nearly a year later these two black sheep, LONGINOS MARTÍNEZ and SENSEVE, with their several grievances, set out together on a journey of their own "para hacer por separado de la Exp$^{n.}$ los reconocimientos y observaciones correspondientes a su profesion" (AGH 462: III 6; V 43).

Somehow they had managed to secure from the Viceroy the proper certificates for travel; which seems to imply tentative approval of their course (or perhaps REVILLA-GIGEDO simply wanted them out of the way). Armed with these they collected payments of salary along the way. They were in Querétaro on January 29, in Villa de León on February 13, in Guadalajara on March 8, were in the Real Presidio de Loreto on March 30, reached Tepic by April 10; further payments were made at Tepic on May 2, June 10, and July 10 (AGH 462: III 2-5, 7, 8, 21, 22. 463: IV 1). In LONGINOS' letter of March 31 from "Loreto, Antigua California," he speaks of owing SENSEVE 480 pesos and of living on the charity of his friends. Evidently such salary as he managed to lay his hands on did not suffice for the expenses of travel and collection.

Meanwhile in Mexico fate, in the shape of a Royal Order, had decided against this adventure.[55] His Majesty was impartially and inflexibly adverse to the substitution of MOCIÑO for SENSEVE and to LONGINOS' claims of independence. The straying members were directed to rejoin the Expedition at once. MOCIÑO was discharged to make SENSEVE's old place available for him. As for the insubordinate one, if he persisted in his attitude he was to be censured, his pay suspended, and himself returned to Spain.

Some time must have elapsed before the order could be transmitted to the west coast. We have already seen how MOCIÑO was sent off to Nootka, removed indeed from the Expedition but continued in its work of exploration and collection. Meanwhile SENSEVE and LONGINOS continued in their independent pursuit of natural history. They were still in Loreto on April 2, April 12, and May 2 of 1792; at the latter date SENSEVE was characteristically petitioning for the double pay due him for field duty; subsequently refused because he was separated from the Expedition (AGH 462: III 1, 16-19). At some time during 1792 the royal threats must have caught up with them; even so, they were remarkably ineffective in speeding their return to the capital. The rains were a convenient excuse for a time; but scarcely explain how SENSEVE managed to continue "en todo esto tiempo en el trabajo y laborio de n$^{tro.}$ destino" until 20 February 1793. He arrived in the capital on March 21.[56] According to MOCIÑO, he had to come home alone almost begging his way "from that enormous distance" (AGH 464: XXII 2; XXIII 31). On July 20 he was ordered to join the Expedition in Córdoba, to which he opposed various rambling objections in his incoherent way, all hinging on the fear that he might not be paid. SESSÉ's order was reinforced by that of the *Fiscal* on November 6 (AGH 462: "III" [IV] 1, 3). Presumably at some time during 1793 SENSEVE rejoined his colleagues, and his share in LONGINOS MARTÍNEZ' mutiny was ended.

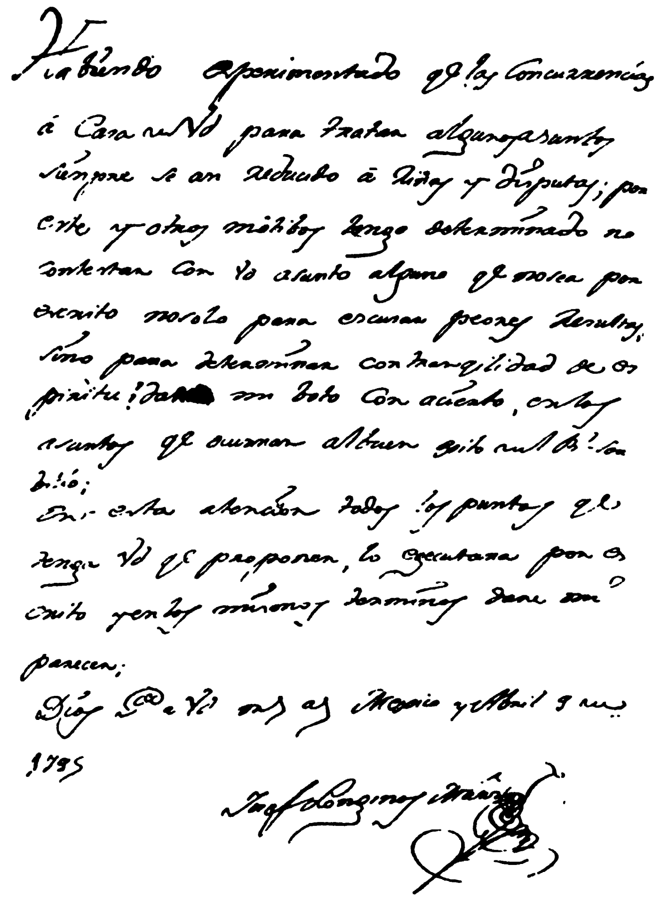
[55] The order was dated 22 March 1791, and received in Nueva España on June 4. AGH 462: V 38, 39. 464: X 10; XXII 1.

[56] AGH 462: III 23, 24. These dates are confirmed by his letter of 17 April 1795, claiming pay for 26 months of travel with Longinos; AGH 462: "III" [IV] 4.

That intrepid individualist continued on his solitary way through 1793. On April 5 he sent two boxes with "producciones de Historia p^a. el R^l. Jardin" of Madrid (AGH 460: 160). In May, when Sessé was again busy trying to add Mociño to the Expedition, he referred to Longinos as 200 leagues away and not likely to join them within two months even if he hurried back (AGH 462: III 32). On June 12 Longinos wrote to the Viceroy from Hacienda de San Josef between San Blas and Acaponeta, acknowledging receipt of orders to rejoin the Expedition and declaring himself "always obedient to the commands of his Sovereign," but calling attention again to Sessé's representations of 1790 from "Cretaro" (Querétaro), which he stigmatized as malicious and untrue.[57] He had not [he went on] stayed in Mexico through dislike of travel. On the other hand he had spent these two years with considerable success in surveying "la Antigua y Nueba California," without regard to the dangers of land or sea and the diseases of that coast; and had collected more than 30 boxes of specimens . . . . The difficulties in the way of joining the Expedition at the moment were very great: the distance of 400-500 leagues; the rains (now conveniently beginning); and the 30 boxes aforementioned. On October 26 Revilla-Gigedo once more suggested that he return to duty, since the rains could no longer interfere (AGH 462: "III" [IV] 6). But Longinos, writing from Compostela on November 6, was offering to survey the entire west coast from Los Angeles to Lima, for the duration of the war (AGH 460: 174). On November 20 he was in Guaristenba (AGH 463: Va. 4), still very ready to execute the royal commands, but desirous of a complete file of testimonials; it was evidently beginning to dawn on him that his unsupported representations were not carrying much weight against constituted authority. The Viceroy replied (December 7) with the now monotonous order that he return to the Expedition (AGH 460: 175).

Just when Longinos returned to Mexico I have been unable to determine. We find him there on 8 May 1794, on which day Sessé wrote asking for the Expedition's copy of the *Systema naturae* to identify some snails. Longinos answered that all that concerned "natural history" was *his* business, and, furthermore, Cervantes had another copy of that work, more complete than his. An acrid controversy at once developed (AGH 464: XXIII 37-49). Sessé's tone was moderate and, at first, conciliatory. He sent the snails to Longinos for determination, after pointing out that the books belonged to the King and that he, Sessé, was responsible for them by virtue of his office. "I love peace [he wrote] and should be sorry to bother His Excellency . . . . though I know that my moderation encourages your pride and contempt for my authority." When, however, Longinos, in his almost illegible scrawl, had told him that disposition of the books should be controlled by a vote, and plainly accused him of ignorance of natural history, Sessé, naturally, became a little warm. "Is it possible [he wrote] that you cannot take up your pen without insulting my patience?" He went on to reproach him not only for the mistakes in Latin in the determinations of the snails (which he had meanwhile received), but for errors in the determinations themselves. "Although I am a poor naturalist, and every day lower in your estimation, yet I know that the species

[57]AGH 463: Va 1. Longinos' spelling is as individual as everything else about him; and his writing—he never used a clerk—almost illegible.



A letter from Longinos Martínez to Sessé, dealing with the controversy of 1795.

"Habiendo experimentado q[e] las concurrencias á casa de Vd para tratar algunos asuntos sienpre se an reducido á rinas y disputas; por este y otros motibos tengo determinado no contestar con Vd asunto alguno q[e] no sea por escrito, no solo para excusar peores resultas, sino para determinar con tranquilidad de espiritu; dar mi boto con acierto, en los asuntos q[e] ocurran [?] al buen exito del R[l] serbitio.

En esta atencion todos los puntos q[e] tengas Vd q[e] proponer, lo executara por escrito y en los mismos terminos dare mi parecer.

Dios gde. Vd. Ms. As. Mexico y April 9 de 1795.

Josef Longinos Martínez".

This may be rendered as follows:

"Having found that attendance at your house for the consideration of any business has always been reduced to quarrels and disputes; for this and other reasons I have decided not to answer you on any matter except in writing, not only to avoid worse results, but to carry on my work in tranquility of soul; and to give my vote, in matters which arise, conducive to the success of the Royal service.

In this connection all matters which you have to propose you will execute in writing and I shall give my opinion by the same means.

God preserve you many years; Mexico, April 9, 1795.

Josef Longinos Martínez."

of the *buccinó* is not Pomum but Perdix; that the Strombus is not a new species but Lentiginosus." The faith of these men in the sufficiency of the works of LINNAEUS for the identification of the fauna of the New World is worthy of note; probably SESSÉ was wrong in his determinations, and probably LONGINOS did have a new species. But LONGINOS' hatred of SESSÉ had now become a monomania, and he was scarcely capable of replying in a rational manner.

On June 3 REVILLA-GIGEDO intervened, asking that each day SESSÉ send him a report on the progress of his work, the individuals present, and the hours they put in.[58] There can be little doubt that this device was aimed at LONGINOS; it was probably made at SESSÉ's request. Next day SESSÉ wrote that everyone had been informed, and that they had worked at his house from 8 to 12 and from 3 to 6, LONGINOS being occupied with the fishes which he had collected, SESSÉ, SENSEVE, and VILLAR in the "coordinacion del herbario," and LA CERDA working on the "most urgent" drawings (MOCIÑO was in Vera Cruz). He promised a daily report "desde mañana." On June 18 came a further request from His Excellency to send him "con la brevidad posible los exemplares q^e. le tengo pedido de las exploraciones de la Exped^n. Botanica de su cargo, con expresion de las producciones naturales q^e. ha adquirido y de lo q^e. ha observado." The next report preserved in the archives, dated June 25, accounts for everyone satisfactorily except LONGINOS, who came after 9.30 in the morning and not at all in the afternoon. REVILLA-GIGEDO promptly demanded an explanation of the truant; but similar reports followed on successive days. LONGINOS, at bay, pleaded sickness "con un fuerte escalofrio y vomito."

LONGINOS' collections are always emerging in the wrangling; it is clear that SESSÉ—whether for his own credit or for the good of science—wanted them incorporated with the other materials gathered by the Expedition, and equally clear that LONGINOS regarded this as an infringement of his rights. A year before he had sent two boxes from the west coast for transmissal to Spain; they had lain in the stores in Mexico all this time, apparently overlooked because the Director had not been informed. On March 5 the Viceroy raised the question of their disposition, and on March 8 SESSÉ asked LONGINOS to come to the Botanical Garden to determine their state. LONGINOS of course replied with excuses, and forbade opening the boxes; whereupon SESSÉ produced an order from the Viceroy. LONGINOS objected that they should be opened in his house, where proper facilities existed; SESSÉ replied that the Botanical Garden was quite suitable, and that he should expect him there at 8 on the next morning. Appended to this last letter is a note; "No asistió, ni contestó" (AGH 464: XXIII 50-59, 75, 76). The boxes were opened and the contents discovered in poor condition; this, of course, lent weight to SESSÉ's contention that all collections should be brought together under his surveillance.

During 1794 and 1795 LONGINOS used the *Gazeta de México* to quarrel with his colleagues. One of his victims was CERVANTES; LONGINOS insisted that the name *Castilla* was contrary to good botanical practice, and that it should be *Castella*. On this and other subjects he was refuted by several of CERVANTES' pupils, who sprang eagerly to the defence of the master. LARREATEGUI wrote, "anyone with only the rudiments of botany

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[58] "que me pase cada dia un parte de lo que se travaja, individuos que concurren a la elavoracion, y horas en que esta se executa." The history of this attempt to ride herd on the Expedition is found in AGH 460: 219-224.

will recognize the crass error or better the bad faith with which the *aficionado* [LONGINOS] confuses the trivial name *elástica* with the specific name, which is still lacking." An interesting comment on the early days of the "binomial system."[59]

Early in 1795 SESSÉ got his extension of time for the Expedition, and began making preparations for his departure for the West Indies. Having received his authorization, he transmitted it to his colleagues, requesting them to come to his house. LONGINOS, of course, refused; "having found [he wrote] that attendance at your house reduces itself to quarrels and disputes, I have resolved not to answer you on any matter except in writing" (AGH 464: XXIII 19-22).

By now a new Viceroy, MIGUEL DE LA GRUA TALAMANCA, Marqués de BRANCIFORTE, was on the scene, and SESSÉ had to detail for him the history of all this miserable business (11 April 1795; AGH 464: XXII 1-4). He pointed out how necessary it was that all should be together for "the arrangement of the work, the collation of observations, the formation of catalogs, and other conferences to ensure that each branch of the Expedition should know of all that had been collected, described, figured, and sent off." LONGINOS regarded his duty as terminated with the six years "allotted by the sovereign," had not been consulted about the proposed extension, had not received any Royal Order for it, and in general was as obstructive as usual. SESSÉ mentioned the Royal Order of 1791, which dealt so severely with LONGINOS' first insubordination; since that time, he said, "never have his disobediences been more frequent nor his replies more insulting"; until finally the former Viceroy had had to threaten him with the castle of Acapulco. In view of all this, SESSÉ went on, it seemed necessary to separate "this individual" from the Expedition and send him back to Spain to give an account of his work. He proposed to replace him by Don ANTONIO CAL and Don JULIAN DE VILLAR PARDO.[60]

LONGINOS also wrote to BRANCIFORTE in his fawning way, setting forth the hopelessness of convincing SESSÉ of his pacific intentions, and intimating that all SESSÉ wanted was to get his hands on his collections and descriptions (April 14 and 22. AGH 464: XXIII 15-18, 25-28). We can only guess what action would have been taken by REVILLA-GIGEDO, always friendly to the Expedition. BRANCIFORTE wavered, not knowing, apparently, whom to believe. SESSÉ had to write again, saying "Don JOSÉ LONGINOS MARTÍNEZ is not such a fool as to put his signature to all the insults of which he has been capable" (April 18. AGH 464: XXIII 35, 36). What these were (he went on) His Excellency could discover from other members of the Expedition who had been present. But on May 18 the Fiscal wrote that there was not sufficient cause for separation; and the Viceroy so informed SESSÉ on June 24 (AGH 464: XXIII 77-83). It was

[59] Gaz. Mex. 7: 280. 30 My 1795. *See also* the supplements for 5 November 1794 and 30 January 1795.

[60] Qualifications of these gentlemen are found in AGH 464: XXIII 64-74. VILLAR had been with "Don Antonio de Pineda y Ramirez . . . encargado del ramo de historia y ciencias naturales por S. M. en el viaje de alrededor del mundo." He was a native of "Villa de Camprobin en la Rioxa"—a countryman of LONGINOS. He had also been in MALASPINA'S expedition, and was at the time employed privately by SESSÉ. We have seen that MOCIÑO, with CERVANTES' help, tried later to add him to *their* expedition. They wrote "not only is this young man skillful with the scalpel, but he is well versed in all branches of zoology" (AGH 465: VII 5, 6). CAL was recommended by GOMEZ ORTEGA and PALAU.

in the same letter that MOCIÑO's petition, to be freed from the embarrassment of LONGINOS' company in his journey to Guatemala, was refused. By the time it was received, however, SESSÉ was in Havana, and MOCIÑO had left for Guatemala.

LONGINOS, after assuring the Viceroy that the coming expedition would reveal his "mucha propension à la buena armonia" (AGH 464: XXIII 84), lingered in Mexico, apparently trying to avoid going at all, and certainly determined not to go with MOCIÑO. He arranged to perform an operation on a cataract which had blinded Don JOSÉ JOAQUÍN DE LECUONA, and advanced this as an excuse for his delay; the Viceroy replied that LECUONA could find another doctor.[61] Probably about the first of July LONGINOS set out on his solitary way. Of his movements we can get a rough idea from a letter written by him nearly a year later in Guatemala.[62] After having surveyed the region of the "Rio Guzaqualcos" (doubtless the Coatzecoalcos, on the Isthmus of Tehuantepec), he had "inspected" the provinces of Soconusco, San Antonio, and Esquintla, as far as Sonsonate, while he "ordered" MOCIÑO to make his way through the mountains so as to increase the territory covered by "our excursions." It is probable that he had caught up with MOCIÑO and LA CERDA in Tehuantepec. From one phrase of his letter it seems that he went from there to Guatemala by boat; he was there in June, long before the others. He proposed to continue down the coast to Nicaragua in 1796, and in 1797 to return to Mexico overland by "la berapas" (Verapaz), El Petén, Campeche, and Tabasco; hoping for a collection of animals better than anything that had ever gone from America to Europe.

The rest of his story is something of a mystery. On the years from 1796 to 1801 the record is silent. LONGINOS may have accomplished his design of exploration farther south; he certainly made collections, in Guatemala or elsewhere. As SESSÉ knew, he suffered from an "infirmity of the breast";[63] it is possible that this restricted his travels and curtailed his explorations. At all events, in 1801 he made up his mind to return to Mexico, and on April 8 requested a passport. He left in the care of ANTONIO GONZALEZ six boxes of his collections, three containing living plants for Spain, one containing birds, one quadrupeds, and one miscellaneous objects.[64]" Some time between then and 29 March 1803 he reached Campeche,

[61] AGH 465: IV 1-4 [sheets 3, 4 are in VI]. The order for LONGINOS to leave for Guatemala, whither the "botanico y pintor" had preceded him, was dated 29 June 1795.

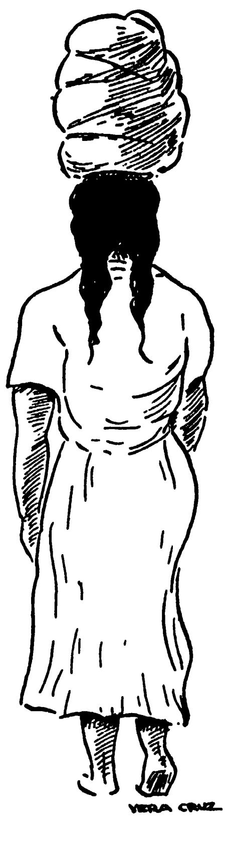
[62] Letter of 3 June 1796; AGH 465: VI 28. "Despues de aber reconocido las cercanias del Rio Guzaqualcos en la costa del norte, y subir por el qince dias embarcado é ynspeccionado en la del sur las provincias de Soconusco, S^n. Anton^o., Esquinta [sic], hasta cerca de Sonsonate, mientras tanto ordene al Botanico D^n. Josef Moziño biniese por el camino de los altos para abrasar mas terreno en nuestras excursiones."

[63] AGH 465: XIV 1. "Ygnorandose el paradero del naturalista D. José Longinos Martínez, y sabiendose por otra parte que hace mas de quatro años se halla gravemente enfermo del pecho, quedarà éste individuo â la disposicion de V.E. . . ."

[64] AGH 465: XVIIa 1, 2. Letter from GONZALEZ dated 3 April 1803, inquiring what was to be done with the effects of the "difunto naturalista D^n. Josef Longinos Martínez." On March 29, he said, there were still in the treasury three boxes, lined with leather, as LONGINOS had left them, and six empty boxes. He appends a list of LONGINOS' belongings: a miscellaneous collection of mostly valueless objects, including a box bed with mattress (valued by LONGINOS at 45 pesos), various tables, chairs, cases, and-trunks, an English saddle and bridle with ornaments and pistols (100 pesos), and other objects whose use it is hard to discover; the pitiful detritus of a solitary and disappointed man.

and there died.[65] SESSÉ never knew what had become of his refractory colleague. LONGINOS MARTÍNEZ had pursued his independence even into the next world.

[65] BERISTAIN, Bibliot. Hisp.-Am. 2: 198. Contr. U. S. Nat. Herb. 23: 15.



THE VOLCANO JORULLO seen by MOCIÑO in 1790 (after a drawing by RUGENDAS in C. SARTORIUS's Mexico, London, 1859)

VIEW OF THE COASTAL REGION ON THE ROAD FROM VERA CRUZ TO JALAPA, trodden many times by the members of the Expedition (after a drawing by RUGENDAS in C. SARTORIUS's Mexico, London, 1859).

Chapter V

CERVANTES: THE BOTANICAL GARDEN AND THE TEACHING OF BOTANY

"Han importado los gastos de este año la cantidad expresada de mil ciento sesenta y nueve pesos cinco reales salvo yerro, lo que juro à Dios Nuestro Señor, y à esta Santa + ser cierto, leal, y verdadero, que no he cargado menos de lo recibido, ni puesto en data mas de lo real, y positivamente gastado, y que si lo contrario resultare, me obligo con mi persona, y bienes à pagarlo con la pena del tres tanto. Mexico tres de Enero de 1793. VICENTE CERVANTES."

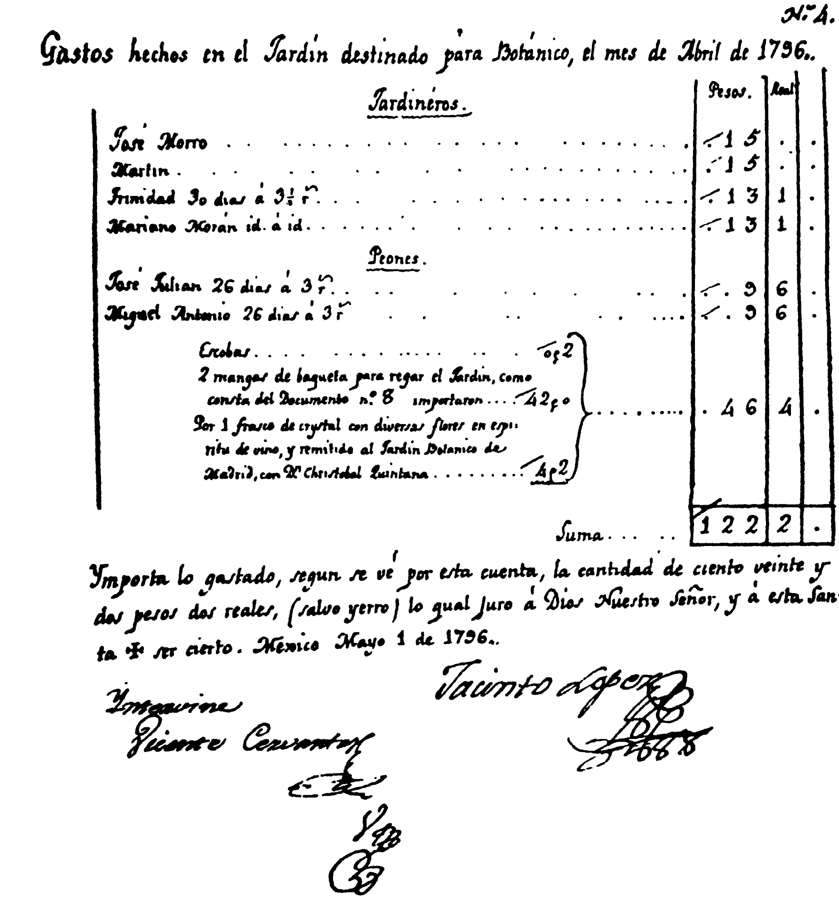
With this attestation and oath the detailed accounts of the Royal Botanical Garden were regularly submitted twice a year; thirty years of the establishment in the Viceroy's palace are faithfully reflected in these documents. Here we may read the names of the gardeners and what they received for their labors; what plants were grown and where they came from and how much it cost to bring them; how SALVADOR the carpenter made a wheelbarrow of cedar and oak for carrying flowerpots and manure; when the lock of the Garden was repaired, when new tools were bought, and when the classroom was fitted out with new curtains and the tables and chairs repainted; what it cost to stage the graduation exercises, to print dissertations, and to make labels for the plants; and many other curious matters.

Soon after the removal of 1791 planting was energetically begun in the Viceroy's garden. In October a cacaloxochitl (*Plumeria* sp.[67]) was purchased for 6 reales, and "el sargento Dn. ANDRES STOLS" spent four days, with a rented horse, bringing plants from El Desierto [de los Leones] (AGH 461: VI 4, 5). In November he was sent to Cuernavaca to select plants for the Garden. A mule was hired to carry him, and he was accompanied by a gardener on a horse; the Garden paid for their accommodations at the inns along the road and for the feed of the beasts for seven days; and it paid also eleven Indians for bringing the plants back. On December 6 more cacaloxochitls were acquired; walnuts also were planted, and aguacates, floripundios, zapote blanco, chestnuts, apricots, figs, oranges, pears .... During succeeding years many plants were imported (*see* Appendix 1). Some of them were Old World species established in Mexican gardens; living plants were received also from the Royal Botanical Garden in Madrid in 1793 and 1795 (AGH 464: XX. 465: VIII). Most, however, came from the country about Mexico and from the forests farther away. Cuernavaca is most frequently mentioned as the source. Such places in the Valley of Mexico as San Angel, Xochimilco, San Agustín de las Cuevas,

[66] "The expenses of this year have amounted to the sum noted of one thousand one hundred sixty and nine pesos five reales, except for error, which I swear to God our Lord, and to the Holy Cross, to be certain, faithful, and true, that I have not charged less than what was received, nor entered more than the amount actually expended, and that if the contrary should appear, I undertake with my person and my goods to repay it with a penalty of thrice as much." AGH 464: XIV 1.

[67] In attempting to determine the identity of plants from native names, I have had recourse to MARTÍNEZ *Catálogo alfabético*.

El Desierto de los Leones, Tlalnepantla, Teotihuacán, Zacatlán de las Manzanas, Ixtacalca, and Chalmas are also frequent in the entries (AGH 461: VI. 463: XVII, XVIII, XIX, XXIV, XXIX. 464: XIV, XIX, XX. 466: II); we read of plants received from "tierra caliente," and once from Campeche (AGH 464: XX. 463: XVIII). The plants came by canoe from such places as "Xuchmilco," by horse or on the backs of Indians from more distant places (AGH 461: VI. 464: XIV. 463: XXIV). As



N.° 4.

Gastos hechos en el Jardín destinado para Botánico, el mes de Abril de 1796.

	Pesos.	Reales	
Jardineros.			
José Morro	15	.	.
Martin	15	.	.
Trinidad 30 dias á 3½ r.s	13	1	.
Mariano Moran id. á id.	13	1	.
Peones.			
José Julian 26 dias á 3 r.s	. 9	6	.
Miguel Antonio 26 dias á 3 r.s	. 9	6	.
Escobas … 0 p 2 2 mangas de baqueta para regar el Jardin, como consta del Documento n.° 8 importaron … 42 p 0 Por 1 frasco de crystal con diversas flores en espiritu de vino, y remitido al Jardin Botanico de Madrid, con D.n Christobal Quintana … 4 p 2	. 46	4	.
Suma	122	2	.

Ymporta lo gastado, segun se vé por esta cuenta, la cantidad de ciento veinte y dos pesos dos reales, (salvo yerro) lo qual juro á Dios Nuestro Señor, y á esta Santa ✠ ser cierto. Mexico Mayo 1 de 1796.

Jacinto Lopez

Ymterviene
Vicente Cervantes

A page of the itemized accounts of the Botanical Garden, signed by JACINTO LOPEZ and countersigned by CERVANTES.

Note, besides wages paid to gardeners and laborers, the cost of two "mangas de baqueta" to irrigate the Garden; and a crystal flask containing various flowers in brandy, sent to the Botanical Garden of Madrid by Don CHRISTOBAL QUINTANA.

late as 1818 a box of living plants arrived from Havana, whence seeds also had come in 1805; doubtless as a result of SESSÉ's collections in Cuba in 1795-1798 (AGH 466: XV. 463: XXIX).

By the end of the year the Viceroy's backyard must have been verdant and flowery. A pretty little garden occupies part of the same space today; laid out in formal beds of various shapes edged with decorative foliage and shadowed by fine trees; but with no attempt at botanical instruction. In

CERVANTES' day it was probably disposed in many small raised beds edged with boards, somewhat in the style of LINNAEUS' garden at Uppsala and other botanical gardens of the time. "Flagged walks, bordered with elegant large pots of flowers," extended between the beds, and "were rendered cool by the creeping plants that are trained over them."[68] In 1805 the paths were repaired with 251 loads of crushed "tesoncle," the volcanic stone much used for construction in the city (AGH 463: XXV). Thyme is mentioned as being used to decorate the walks (AGH 463: XIX). Some 1400 species were cultivated; all labeled with metal tickets (AGH 466: VIII 15). From the examples above, there would seem to have been an undue emphasis upon fruit trees; but doubtless the beds were full of herbs from the Old World and of native plants brought in from the surrounding country. We know that new and unknown plants often came in for identification or admiration, and some were doubtless cultivated (AGH 464: XX). One of the recurrent items is carnations (*clavellinas*). Sixty-seven dozen came from Ixtacalco on one day in January 1792, followed by 36 dozen on another day, 22 on another, and still more in February (AGH 461: VI. 464: XIV). The modern visitor to Mexico still admires the generous bunches which come from the *chinampas* of Xochimilco, and are purchased for a few cents. Whether or not these were planted, their presence in a botanical garden needs explanation—together with the 97 rosebushes and 18 dozen bachelor's buttons. CERVANTES in a letter of 26 June 1817 wrote of the necessity for the replacement of certain plants which were used in the lessons, as well as *those which adorn the beds* (AGH 466: unnumb. file 8). Flowers were purchased in 1794 (and at other times) for "los ramos de S. E." on St. Michael's day; that is, for wreaths or branches carried by His Excellency the Viceroy or used to adorn a chapel in which he attended service (AGH 464: XIX. 465: II). They were used also for the decoration of the hall in the University where the graduation exercises were held (AGH 464: XX). Perhaps flowers grown in the Botanical Garden also were used for such purposes. It is possible that the Garden (like other such establishments) was used for the private gratification of persons in authority. In speaking of its later decay, CERVANTES wrote (24 March 1813), "The *cenador*, which His Excellency the Count of REVILLA-GIGEDO caused to be built at a cost of more than 2000 pesos, is ruined, and many of the curious vines which adorned it have been lost. These were the admiration of the celebrated botanists and naturalists Baron VON HUMBOLDT and BONPLAND when they visited this Court [in 1803]" (AGH 466: VIII 13). After all, it *was* the Viceroy's garden. In 1807, the *Fiscal* complained that the gardeners were selling plants to the public; this should be stopped as contrary to the ends of the establishment (AGH 466: IV 22-26). CERVANTES replied that "all the flowers in the Garden are not worth, in my opinion, 4 pesos, and I can assure Your Excellency in all frankness that I would not give that much for them." He denied that any malpractice was going on. Occasionally, he wrote, the gardeners had made "ramos" for some altar or dinner table, but these did not come from the Garden but were bought "en la acequia."[69] The *Fiscal* answered that the evidence was

[68] BULLOCK, Trav. Mex. 183. This was thirty years later.

[69] *i.e.*, Calle de la Acequia, the street which bordered the Palace and the Garden to the south. It was named from a canal whose course it followed, and which was covered over during the administration of REVILLA-GIGEDO (GALINDO, Hist. Sum. Mex. 176).

incontrovertible and that CERVANTES was being deceived. It is clear from the records that the Garden did some traffic in flowers, for whatever purposes they were used, and whether officially or not.

In December 1787 SESSÉ and his colleagues had gone to Toluca to see the famous tree known to the natives as Macpalxochiquahuitl and to science as *Chiranthodendron pentadactylon* (Sterculiaceae); SESSÉ referred to it as the *arbol de las manitas*. The Indians regarded this tree with superstitious awe, believing it the only one of its kind; to propagate it, they said, would offend the gods. They carefully gathered all the flowers before they could form fruits and SESSÉ's efforts to enlist their help in allowing some to mature or in rooting some branches were futile.[70] The botanists, however, made a number of cuttings, three of which survived, and from one of these a tree was successfully grown in the Garden. In October 1794 a cart was hired to carry branches of this tree to Chapultepec; at this time, apparently, an effort was still being made to plant Chapultepec as part of the Botanical Garden, CERVANTES being directed to attend to this while SESSÉ was absent in Cuba.[71] In May 1797, 72 pesos 3 reales were spent to "gravar y estampar" this species (AGH 463: X). It is probable that this entry refers to the printing of the discourse on the tree given by CERVANTES at the opening of the course in that year.[72] Other individuals were later discovered in Guatemala, probably by MOCIÑO.[73] In 1848 the tree was still standing in the garden of the Palacio; and from it another was raised which lived until 1935, when it succumbed to a disease; the third generation grows there now.

In October 1791 the Garden was tilled by four gardeners, named MARIANO HILARIO (or YLARIO), MARTIN DE LA CRUZ, JUAN DE LA CRUZ, and MARIANO DE LA CRUZ. Of these the first two had worked in "El Sapo" from the first and had helped with the moving; MARIANO HILARIO stayed for four years more (AGH 461: VI 1, 2, 4, 5. 464: II 1, 9). There were also two peones, JOSÉ DE LOS SANTOS and ANTONIO TRINIDAD. The gardeners received 3½ reales a day, the peones 3. Subsequently the gardeners were reduced to three; they were assisted by peones varying from two to seven. All these were supervised by the head gardener, JACINTO LOPEZ, who, as has been seen, arrived in 1790 from the Royal Botanical Garden of Madrid. He served in Mexico for 24 years.

The original plan of the Garden included the training of apprentice gardeners (AGH 466: VIII 20-33). In 1790 the Viceroy was instructed to appoint youths to serve in this capacity at 150 pesos a year "para que asistiendo al Jardin Botanico de Mexico, se instruyan en el Arte de Jardineria, y puedan despues ser utiles en este Ramo" (AGH 463: III 1). This was apparently never done, perhaps for want of candidates. CERVANTES constantly was hampered by the lack of skilled help in the Garden,

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[70] The story is told by CERVANTES *in* An. Ci. Nat. 6: 303-314 (0 1803), and by BONPLAND *in* Pl. Aequin. 1: 85, 86 (1806). CERVANTES named it *Chirostemon*, and BONPLAND *Cheirostemon platanoides*; but CERVANTES seems to have published the original name in a pamphlet in Mexico, according to BONPLAND. For further data I am indebted to Professor MAXIMINO MARTÍNEZ.

[71] AGH 464: XIX. Other trees had been removed thither in January.

[72] BONPLAND says that this discourse was read by LARREATEGUI on 1 June 1795; the contemporary notice (*see below*) shows that LARREATEGUI did speak on that occasion, but on another subject.

[73] BONPLAND says only by a pupil of CERVANTES; which information was given him by the latter.

and apparently there was little prospect either of inducing sons of the upper classes to take up horticulture as a profession, or of teaching the Indians enough so that they attained real proficiency. His native helpers did not avail themselves of instruction, and were never anything but laborers.[74] Occasionally one of the peones was promoted to be a gardener, but the gardeners rose no higher. In 1800, in one of the interminable disputes between CERVANTES and the royal auditors over the accounts, it was suggested that the gardeners ought to make the copies of the catalogue of the plants in the Garden instead of the clerks who were specially hired to do it; further that they ought to sign receipts for their wages, like everyone else.[75] CERVANTES rather indignantly replied (10 May 1801) that for 3½ reales a day one did not get men who could read and write!

In 1800, the declining fortunes of the colonial government being in the hands of MIGUEL JOSÉ DE AZANZA, it was suggested that LOPEZ had now accomplished the "establishment" of the Garden, and the six years allotted for that purpose had elapsed; he should therefore return to Madrid (AGH 464: III 12). This idea arose in a very natural misconception of the nature of the Garden and the duties of its staff. The Botanical Garden was founded by the same Botanical Expedition to which six years had been assigned to survey the "natural productions" of New Spain; CERVANTES and LOPEZ were entrusted with the task of "establishing" the Garden; after this had been accomplished, the institution was presumably to be run by native professors and gardeners. As we shall see, the status of CERVANTES himself was later called to account. At this time, the prospect of attempting to continue to run the Garden (as he assumed he would) without any experienced helper filled him with consternation. SESSÉ, to whom the proposal was sent, replied with his usual clarity and energy that in that case there would be no one to do the work. He called attention to the fact that the regulations for the establishment of the Garden provided for an assistant to the head gardener, who had never been appointed. So cogent were his arguments and so well established was the Garden among the cultural institutions of Mexico that he carried his point. Not only did LOPEZ remain but an assistant was appointed for him: another LOPEZ, JUAN ANTONIO, was named *ayudante* on 22 April 1800 (AGH 465: X 6, 7). He served only for four or five months, however.[76] He was a native of Madrid, and had had experience in Havana. The appointment cost CERVANTES the services of the two remaining peones, which were never regained (AGH 463: XIV 14, 15).

Every year, usually in June, the course in botany was opened by CERVANTES. Notices were printed and distributed through the city; necessary repairs were made to the *aula*: occasionally the portrait of the Virgin was taken down and cleaned. CERVANTES himself gave the inaugural discourse on the first of these occasions, 2 May 1788, his subject being the his-

[73] Letter of 8 March 1799: "El exito no correspondió à nuestras miras; por que descuidando mucho la instruccion que debia promoverlos à dha. plaza, no se adelantaron nunca à los demas peones, y asi quedaron siempre en clase de tales." AGH 463: XIV 14, 15. Also letter of 19 June 1813 in AGH 466: VIII 15.

[75] AGH 463: XVII 12. "Two quires of white paper" had been purchased for this manuscript list, which must be the *Hortus mexicanus* cited by BERISTAIN (Bibliot. Hisp.-Am. 1: 337).

[76] AGH 464: III 19. In 1802 SESSÉ wrote that the vacant position of *ayudante* had never been filled by a proper person ("algun hombre de buena disposicion." AGH 465: XIV 1).

tory of botany. He told of the knowledge of this science held by the ancients, the increase which resulted from the labors of the "founders," what was added in the "systematic epoch," and the progress made since the reforms introduced by CAROLUS LINNAEUS.[77] He gave a clear exposition of LINNAEUS' system and its advantages. Finally he read the plan of instruction furnished for the Garden, so that all might know their privileges and thus be stimulated to the study of this most useful science.[78] This was in CASTERA'S house, called in the published version of the lecture "la sala del nuevo Real estudio de Botánica . . . . de México." The following year, on May 4, CERVANTES again opened the course, speaking on the value of method in studying plants.[79] On 28 May 1791 he discoursed on the medicinal plants of the vicinity of Mexico.[80] On 1 June 1793 the opening lecture, on the same subject, was given by MANUEL MARÍA BERNAL, who had graduated from the course in 1792.[81] On 2 June 1794, CERVANTES gave his well known account of the "arbol del hule," naming it *Castilla elastica*.[82] The resulting controversy over the name was published in contemporary periodicals and aired by LARREATEGUI at the graduation exercises of 1794.[83] CERVANTES and his students were much interested in "la goma (resina) elástica" which was obtained from this tree; a bottle of the "jugo lechoso" had been sent from Jalapa by Dr. DANIEL O'SULLIVAN in May 1794 (Naturaleza 7; apend. 24n). On 1 June 1795 JOSEPH DIONISIO LARREATEGUI, a graduate of 1794, spoke on "the true method of describing a plant."[84] On 4 June 1796 IGNACIO NAVAMUEL, "cursante en Medicina y Discípulo de dicho Jardin," gave a lecture on "Recent discoveries in the obscure family of the mosses" (Gaz. Mex. 8: 111). Probably in 1797 the course was opened by the discourse on the *arbol de las manitas* already noticed. CERVANTES again appeared on 3 June 1798, with an address on Violeta estrellada (*Ionidium polygalaefolium* Vent.) and its virtues.[85] In 1801 he had the

[77] His organization of botanical history followed, in the main, that of LINNAEUS; this was expounded in the *Philosophia botanica*, then still a relatively new textbook.

[78] Gaz. Mex. 3: 77 (supl.). 6 My 1788. *See also* LEÓN, Bibl. Bot. Mex. 86; COLMEIRO, Bot. Hisp.-Lusit. 12; Mem. Lit. Madrid, January 1789.

[79] COLMEIRO, Bot. Hisp.-Lusit. 12. LEÓN, Bibl. Bot. Mex. 86.

[80] COLMEIRO, Bot. Hisp.-Lusit. 41. LEÓN, Bibl. Bot. Mex. 87. According to COLMEIRO, this was published in Puebla in 1832 as *Ensayo para la materia médica mexicana*. It is cited by BERISTAIN (Bibliot. Hisp.-Am. 1: 337), and mentioned also by GARCÍA RAMOS (Bol. Soc. Mex. Geog. Estad. II. 1: 760). LEÓN notices a reprint of 1879.

[81] LEÓN, Bibl. Bot. Mex. 85. Gaz. Lit. Mex. 3: 129-150. An entry in the accounts for June of this year reads: "Por las copias del Discurso con que se hizo la abertura de las lecciones p$^{\text{r.}}$ Madrid y la impr$^{\text{ta.}}$ . . . . . 8 pesos." For the graduation of various of CERVANTES' pupils, *see* Appendix 2.

[82] This was printed in 1794 (An. Ci. Nat. 7: 214); and again in May 1797 (AGH 463: X). *See also* LEÓN, Bibl. Bot. Mex. 85; Gaz. Lit. Mex. 2 Jl 1794, 1-35. It was reprinted in Naturaleza 7: apend. 18-33. It formed the basis of an anonymous article (by CAVANILLES?) entitled: "Observaciones sobre algunos vegetales que producen resina elástica." This appeared in An. Ci. Nat. 2: 124-128 (June 1800).

[83] LONGINOS MARTÍNEZ' part in this wrangle has already been noticed (Chapter IV). GOMEZ ORTEGA wrote, "No acierto á resolverme si preferiria la terminacion Castilloa á la Castilla" (Gaz. Mex. 7: 286n). The editor of the reprint in La Naturaleza wrote, "Creemos que por una errata de imprenta dice en el original Castilla, debiendo ser *Castilloa elastica*" (Naturaleza 7: apend. 18n). Whatever were CERVANTES' intentions, *Castilla* was the published form; had it been really an error, he had ample time to correct it publicly.

[84] Gaz. Mex. 7: 302. 20 Je 1795. Bonpl. Pl. Aequin. 85, 86. 1806.

[85] COLMEIRO, Bot. Hisp.-Lusit. 46. An. Ci. Nat. 6: 185-199.

pleasure of welcoming back to the classroom his favorite pupil José Mociño, recently returned from his exploration of southern Mexico and Guatemala; he spoke on medicinal plants of Mexico.[86] The following year, on June 2, Luis Montaña, professor of medicine, spoke on the empirical (or scientific) method in botany (An. Ci. Nat. 6: 199-222). Notice that these lectures, which must reflect to some extent what was taught in botany in those days, are concerned either with "pure" science, or with medicine; never with agriculture or the other arts to which botany may be applied.

"Some have contented themselves with treating of the nomenclature of plants based on external structure. Others add their virtues and medicinal and economic uses. The first without other object would be useless or a mere curiosity. The second is the aim of botany."[87] With these words Gomez Ortega began the *plan de enseñanza* which he drew up for the new botanical garden. The studies were divided into the theoretical and the practical. The former were concerned with the parts of plants, their internal structure, their propagation, their virtues, and the manner of preserving them. The practical work began with the seven natural families established by Linnaeus and their subdivision into classes, orders, genera, and species. The program included "los exercicios literarios q^e· son el alma de las lecciones." Each lesson was to be divided into two parts. First, the preceding lesson was recited by a pupil named by the professor; second, the professor explained the ensuing lesson, giving each student a branch or specimen of the plant under discussion. We must admire this early example of the "laboratory-demonstration method." The class was to meet three times a week for lessons of two hours each. The last day of each week was to be devoted to questions (*dudas*); one pupil presenting a summary of the week's work. Demonstrations in the Garden and excursions in the surrounding country were to alternate with formal class work, and serve for review. As textbook they had the *Curso elemental* of Gomez Ortega and Palau y Verdera, which had been published in 1785 for the classes in the Royal Botanical Garden of Madrid, and was reprinted in Mexico in 1788. They used also Linnaeus' *Species plantarum* and *Systema naturae*.[88]

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[86] An. Ci. Nat. 5: 288-296. N 1802. *See also* above (Chapter II).

[87] AGH 466: VIII 28. There can be little doubt of the authority of these words, since the plan here outlined closely resembles what was practised in the Royal Botanical Garden of Madrid at that time. It was part of the *Reglamento* which prescribed all the activities, duties, and privileges of the members of the staff. It was incorporated in the Royal Order of 23 November 1787 (AGH 462: Ib 42-53). In another place (462: unnumb. file) it is written that "los objetos combinados del R^l· Jardin botanico son la instrucion [sic] de los profesores de medicina, cirugia y pharmacia, y la colecion de plantas utiles al arte de curar y a otras, p^a· su propagacion y p^a· acopio de semillas q^e· remitir a S. M." The text ("parte teorica") was for sale in the *librerias* of the Calles de Santo Domingo, Capuchinas, and "la de esta oficina" [Gaz. Mex. 3: 82 (supl.). 6 My 1788].

[88] A copy of the *Species plantarum* was purchased in March 1793, and a "Systema de vegetales" in April of the same year (AGH 464: XX). Two copies of the *Systema naturae* are referred to in the controversy of 1794 between Sessé and Longinos (*see* above, Chapter IV).

Among Castillio's effects were the following. "Practica de Linneo traducida por D. Antonio Palau . . . . Hernandez Historia Plantarum Mexicanarum . . . . Tournefort . . . . Jacquin Americ . . . . Filosofia Botanica Linn. . . . Yd. traducida por Palau . . . . Curso elemental de Botanica . . . . Systema Naturae Linn. . . . Genera Plantarum. . . . Hist. Botanico-Practica de Morandi. . . ." (AGH 464: XVI 35).

On 20 December 1788, at the end of the first course, three pupils sustained the public exercises or *acto de botánica,* under the direction of their master.[89] The three were "el Dr. Don Joseph Vicente de la Peña, Medico acreditado en esta Corte, D. Francisco Giles y Arellano, Pasante de Cirugia en el Hospital Real de Indios, D. Joseph Timoteo Arzinas, Practicante de Farmacia." These, with the general applause of those present, together reviewed the whole content of the doctrine of the course, which they then defended against the attacks of eight persons conversant with botany (*aficionados*) and of one pupil. After this they determined and described plants drawn by lot from the many which adorned the hall, most of which were unknown to them, being from *climas calientes* and only brought in that day. They distinguished not only the genus and species of each, but also their uses and virtues, with their Mexicans names and the meanings of these. The following were the plants presented: *Bignonia occidentalis, Loranthus americanus* (quauhchitli), *Cerbera thevetia* (yoyotl), *Gentiana violacea* sp. n., *Mimosa esculenta* sp. n., *Datura maxima* sp. n. (tecomaxochitl), *Delphinium ayacis, Euphorbia fastuosa* sp. n. (flor de noche buena), *Ageratum strictum* sp. n. [Under these slightly garbled names the modern botanist may recognize several now familiar species.] GAMBOA presided, as he had at the opening of the course, having with him all his ministers, and the "asistencia de ambos Claustros, crecido número de Religiosos, Militares, y principal Nobleza de esta Capital," all of whom enjoyed "the interest and pleasure of the doctrines which were aired in the three hours and a half that the exercises lasted, as they did the decorations, the illuminations, and the music which occupied the intervals during which plants were distributed." Admiration was expressed for the Professor, who had attained such results "without other help than that afforded by the few plants of this barren region and those contained in the small garden of Castera."

The dissertations presented by these students and their successors had been formulated by CERVANTES and printed at the expense of the Garden (AGH 463: I 1). They were based on the theoretical part of the course, and the regulations prescribed that they include the classification of LINNAEUS—thus transformed into dogma. As we see from the above account, the student's presentation was followed by questions which he had to answer. At least three plants must then be determined and described; it is necessary to recall that an important part of botany in those days, as outlined by LINNAEUS, was the proper description of a species, which was also its *specific name,* and followed more or less definite rules. Prizes of 50 pesos each were awarded to outstanding students.

During the rest of the year (December to May) it was suggested that an hour be set aside every Thursday afternoon to answer questions and to continue the practical work, especially with useful plants; similar office hours should be kept if possible on Sunday afternoons, since at that time students could come without interrupting their regular occupations. "La

[89] This account is taken from Gaz. Mex. 3: 213-215 (6 Ja 1789). The same ceremony for the following year is described in Gaz. Mex. 3: 439 (22 D 1789); it was signalized by the participation of JOSÉ MOCIÑO, with JOSÉ MALDONADO and JUSTO PASTOR Y TORRES. According to COLMEIRO, the *Ejercicios* were published each year (Bot. Hisp.-Lusit. 119); one may read in the titles cited the names of the graduates in many of the years (*see* Appendix 2). I have been unable to discover what happened to the course which was begun as usual in 1791, and which must have been interrupted by the move to the Palacio Real; no record of an *acto* appears in the accounts for this year, or for 1790. Both these years are missing also from the titles cited by COLMEIRO.

vida del hombre es corta pa. esta estudio." Such opportunities would also [it was said] tend to distract young fellows from the disorders to which youth is prone on these afternoons.

In 1790 CERVANTES was pleased and honored by the request of two members of the faculty of the University for instruction and examination in botany.[90] These were Don JOSEF GRACIDA, Professor of Anatomy; and Don DANIEL O'SULLIVAN, Doctor of Medicine, whom we have already seen collecting latex near Jalapa; both were among those who examined MOCIÑO in the exercises of 1789 (Gaz. Mex. 3: 439). These gentlemen spent three or four hours in CERVANTES' house every evening for more than two months, not being free at other times. But when CERVANTES was about to submit the dissertations, which they were to defend, to the Viceroy for approval, so that they could be printed, he learned that the candidates had been forbidden to attend the exercises, since it was contrary to the constitution of the University "for any doctor or professor to be presided over by any other." This, CERVANTES found, was merely an excuse, for his offer to step down from the chair during the exercises was of no avail. The real reason was a prejudice against the subject, and, still more, a jealous fear lest graduates in that science might, on the strength of their training, be preferred for appointment to His Majesty's service. CERVANTES pointed out that it was no disgrace to the University—it should not be forgotten that in Spain Doctors of Medicine and of Laws, soldiers of high rank, and clerics had sustained corresponding exercises, which had been publicly praised. But GRACIDA and O'SULLIVAN did not graduate in 1790; in fact, the exercises were apparently omitted that year.

On 23 December 1790 the attorney general proposed a confidential inquiry to mediate between CERVANTES and the University. The following year, according to CERVANTES' letter of October 24, GRACIDA continued to attend lessons and was planning to graduate at the regular time, CERVANTES, as he had suggested, vacating the chair. Three other pupils[91] were to appear; they had been coming to CERVANTES' house for a month or more to prepare themselves, perhaps because of the move to the Palacio Real. CERVANTES suggested that if the new *aula,* in the gallery of the Palacio, were ready by mid-December, the exercises could be held there, and disputes with the University might be avoided. Either the *aula* was not finished, or the affair was postponed for other reasons. In 1793 SESSÉ appealed to the Viceroy to order the celebration of the omitted exercises, "to protect this useful establishment, especially in view of the good pupils that the Professor has had, such as Don JOSÉ MOCIÑO and Don JOSÉ MARÍA MALDONADO." The dispute was still going on in 1794, when EUGENIO DE LLAGUNO in Spain, in a letter to the Viceroy, agreed with CERVANTES that in no way did it prejudice a doctor or professor of the

[90] The records of this business are found in AGH 463: I 1-8.

CERVANTES had hoped that the graduation and learning of these two students would do much to dispel the "false impressions which some persons entertained of the new institution, of which many traces remain even among those who were at first most concerned with its establishment. . . ." ("sugetos ambos que por su graduacion y doctrina vean mui del caso para borrar de una vez las falsas impresiones, que causó en algunos este nuevo establecimiento, y de que duran aun muchas reliquias en los que se mostraron mas preocupados en el principio de su ereccion. . . . AGH 463: I 1).

[91] SEBASTIAN MORON, Practicante de Farmacia; PEDRO PRIETO, Profesor de Medicina; and FRANCISCO BALMIZ, Profesor de Cirugía. MORON had been among the examiners in the exercises of 1789 (Gaz. Mex. 3: 439. 22 D 1789). BALMIZ was the author of a work on *Agave* and *Begonia* which appeared in 1794.

University to be examined in another science by another professor. I can find no record of the actual graduation of these victims of institutional jealousy. But the crisis which threatened the success and good name of the young institution was passed; from 1793 on graduation exercises proceeded in an unbroken series for many years.

A somewhat similar quarrel with the *Tribunal del Protomedicato* is alluded to by some letters of November 1792 (AGH 464: XIII). It appears that FRANCISCO GILES Y ARELLANO, whom we have already met in the exercises of 1788, had never been examined by the *Tribunal* and had not received his official title. CERVANTES wrote that the president of that body had excused himself from being present and had made it impossible for the necessary formalities to be completed. Presumably this trouble also was somehow smoothed out.

In the maintenance of any garden, the supply of water is one of the principal problems; this is particularly true where all the rain falls during one season. For most of the year the Botanical Garden had to depend upon water drawn from wells by hand and distributed to the beds through pipes or troughs. The latter were in constant need of repair and renewal. In March 1794 we read of the expenditure of 51 pesos for 48 yards of new pipe[92] at 8½ reales a yard; in October 48 yards more were purchased. Similar entries appear for April 1796 and for subsequent dates.[93] The pipes were apparently made of leather held by withes or wicker. Several years later it was estimated that this system cost the Garden 250 pesos a year in labor, besides about 100 pesos for the replacement of equipment (AGH 464: III 20. 465: X 4-6). In 1795 CERVANTES asked for a tank or basin, to be placed in the center of the Garden on the spot occupied by a ruined wooden house or hut. This structure had been build by BUCARELI as a refuge during earthquakes (AGH: 462: Ib 112, 113, 122, 123). For those who believe that "bureaucracy" or "red tape" is a recent invention of a major American political party, it may be instructive to follow the history of this proposal until it was realized—nine years later.

On 7 December 1795 the *Fiscal* asked CASTERA to look into the matter. On 22 April 1796 CASTERA submitted his estimate and recommendations. "Since we are not immune from the same fear" of earthquakes, the house was worthy of preservation. It was scarcely a ruin—though the roof was gone—and could be repaired at a cost of 400 pesos. The situation (he wrote) was not appropriate for a tank, which should stand in a corner; this architect was evidently unfamiliar with the design of the famous gardens of the Alhambra, with their central pools, and with gardens of the Old World generally. So nothing was done. The matter came up again in 1799, in connection with filling the position of *ayudante* (AGH 463: XIV 14, 15). SESSÉ wrote on April 2 of that year, pointing out that the construction of the tank would save the labor of two peones, besides the cost of repairs (AGH 465: X 4-6). It will be recalled that SESSÉ succeeded in bartering two peones for the services of JUAN ANTONIO LOPEZ; but he did not get the tank, and subsequently lost LOPEZ also. On 28 March 1800 CERVANTES wrote that after the "recent temblors" the building was next

[92] "Una manga de baqueta para regar la huerta." AGH 464: XIX.
"Mangas de baqueta para conducir el agua â los quadros" AGH 465: X 4-6.

[93] AGH 463: VIII; XI; XVII; XIX. In 1801 the varying cost of this item was questioned by the auditors; to which CERVANTES replied that "estas piezas aumentan ó disminuyen de valor, segun son mas, ó menos largas, y segun baja ó sube el precio de los cueros . . . ."

to a ruin (AGH 462: Ib 124). On April 12 the matter was referred by the *Fiscal* to the Viceroy and by him transmitted to "Don Cosme de Atier y Trespalacios, Caballero de la R$^{l.}$ y distinguida Orden Española de Carlos Tercero, Ministro Honorario del R$^{l.}$ y Supremo Consejo de Indias, Didor Decano de èsta R$^{l.}$ Audiencia, Juez Superintendente de las Obras de dicho Real Palacio."[94] This dignitary in turn referred it once more to the architect CASTERA, who, we may suppose, drew a new fee for reiterating (1 June 1800) his previous findings. On September 23 demolition of the house was authorized, and on December 9 the *Fiscal* assented to this and to the construction of a tank, provided the estimate of the cost made by CASTERA in 1796 were approved by the court. On December 20, the cost having been approved, he requested the Viceroy to order the demolition of the hut and the construction of the tank. The entire *expediente* was then unfortunately passed (December 23) to the *Protomedicato,* since it had apparently occurred to some one to question the salubrity of the proposed arrangement.[95] Certain "poor invalids" were lodged next to the Garden, and the corner in which it was proposed to build the tank was next to their *cuartel.* The *Protomedicato,* never averse to doing the Garden a bad turn, returned the verdict (9 January 1801) that the resultant humidity would be detrimental to their health (AGH 462: Ib 143). The file then went back (January 21) to the *Fiscal* for his suggestions; he (January 26) reviewed the entire proceedings (AGH 462: Ib 146, 147). The tank, he said, would be prejudicial to the health of the invalids, since "dropsy, diarrhea, and other illnesses" result from humidity. The house, on the other hand, was an eyesore ("causaria deformidad, embaraso y mala vista"), as had been established by CASTERA's report. (Presumably no other person, the *Fiscal* least of all, could be expected to *look* at the structure and see whether or not it was actually ruinous.) As a result of all these protracted legal proceedings, the hut, he said, should be demolished, the space thus acquired should be added to that available for growing plants, and the timber salvaged be used in the construction of the beds. Demolition was accordingly ordered on February 7 and was accomplished by November (AGH 463: XIX). But no tank!

The water problem was reopened in 1803, when it was proposed to return LOPEZ to Spain. On February 4 SESSÉ called attention to the fact that plants were dying from lack of water (AGH 464: III 19, 20). In the following year, in a report by ANTONIO VELASQUEZ to the Viceroy, it was pointed out that the insalubrity of the proposed tank was the more doubtful in that the invalids already had such a tank themselves in which to do their laundry (AGH 462: Ib 170-173). Thus were the professors and

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[94] AGH 462: Ib 128. The attorney's letter of transmissal is worth quoting as a masterpiece of verbiage: "El Fiscal de R$^{l.}$ Has$^{a.}$ dice que p$^{a.}$ calificar el ruinoso estado de la casa de madera que se halla en el Jardin de este R$^{l.}$ Palacio, seg$^{n.}$ expone D$^{n.}$ Vicente Cervantes en la representacion que precede, se servirá V. E. mandar pase el exped$^{te.}$ al V. didor decano Juez Superintend$^{te.}$ de las obras de él, p$^{a.}$ las dilig$^{as.}$ propias de su inspeccion, p$^{r.}$ medio del Maestro mayor D$^{n.}$ Ygnacio Castera, que reconociendola exponga si en efecto está inutil, si podrá repararse, ó el aprovechamiento que tendran los materiales, como tambien el sitio q$^{e.}$ ha de quedar desembarasado en el primer caso, y la cantidad q$^{e.}$ importará el reedificio en el segundo, con lo demas q$^{e.}$ se le ofrezca sobre la representacion de Cervantes, y en vista de lo q$^{e.}$ informe de resultar el V. D$^{n.}$ Cosme de Atier, pedirá el Fiscal lo q$^{e.}$ estime correspond$^{te.}$" A sentence that would put to shame even those of a modern journalist!

[95] AGH 462: Ib 142. It is interesting to notice that the present garden adjoins a clinic and other facilities of a department of public health, and is generally patronized by "poor invalids."

architects, and the ponderous ratiocinations of the *Fiscal,* confounded by simple common sense! Finally, on 23 June 1804, the *Fiscal* ordered that the tank be built, with strict attention to the utmost economy (AGH 462: Ib 174-176). In 1806 it was evidently in use, since a repair to it is mentioned in the accounts; also repairs to two wells and to a pump (*bomba*) (AGH 463: XXVIII). Perhaps the latter was the chain-pump (*noria*) built by LOPEZ and mentioned in 1807 and in 1818 (AGH 466: IV; XVI 1). For the water supply remained inadequate, and remedies were still being discussed. Two hundred plants were lost through lack of water in 1817 (AGH 466: unnumb. file 3). Since the water supply of the Garden was then supplying the Viceroy's kitchen, the secretary's house, and the barracks of the regiment stationed in the palace, a certain shortness is not surprising. In fact, the supply was investigated, pronounced sufficient but wastefully used (AGH 466: XVI 5).

But it was already too late; the reform in the method of supplying the plants with water came only when the Garden had been greatly reduced in size, when the money allotted to its support had been cut nearly in half, and when, in fact, it was already doomed. The decline in its fortunes began shortly after SESSÉ's departure for Spain. This must not be taken as a reflection upon CERVANTES, who continued for many years thereafter to labor with his accustomed faithfulness and integrity; the energy and prestige of SESSÉ could at most have served to retard the decay. The Viceroy and his officers may be forgiven for lack of attention to the needs of the Botanical Garden; other needs were more pressing. Their own fate was nearer than that of the cultural institution.

SESSÉ left for Spain, with his family, his retinue, his colleagues, his collections, early in 1803. In March 1802, being then ready for departure except for details of transportation, he wrote to the Viceroy about the status of various members of the Expedition (March 14; AGH 465: XIV 1-4). CERVANTES, he said, preferred to continue the destiny of the Chair in the Garden, and there was no reason to relieve him of it, since the royal orders referred only to the return of the members of the Expedition, who must now give an account of their discoveries, and since no one was better qualified to occupy the Chair; also since if he were retired, the Treasury would be obligated to pay him half his salary anyhow. For like reasons LOPEZ should be continued as head gardener. Furthermore, although by his own departure the position of Director of the Garden became vacant, he intended after some time to return to Mexico permanently. And since the *Tribunal del Protomedicato* had no one instructed in the elements of botany or in the least capable of substituting for CERVANTES if he should be ill or otherwise absent, pending disposition of the place of Director by His Majesty, Don LUIS MONTAÑA, the most learned and accomplished professor available, should be appointed provisionally. This last suggestion was apparently never taken up; but CERVANTES became *Director* as well as *Catedrático* in botany.[96]

[96] AGH 465: XIV 17-24. Discussion of this matter occupied about a year. They must, however, have anticipated the final decision of the *Fiscal,* since by that time—23 March 1803—the party for Spain was made up and about to leave, if not already on its way.

In 1801 CERVANTES wrote that the place of *Yntendente* and *Director* of the Royal Botanical Garden at Madrid had been suppressed, and Cavanilles made "unico profesor, y encargado del govierno, y direccion del jardin."

The *Reglamento* for the Garden contains instructions for the selection of a new Professor by competition (AGH 466: VIII 20-33).

On 2 April 1811 CERVANTES wrote that part of the Garden had been taken for the construction of a new barracks; a change which accurately reflects the changing temper of the times (AGH 462: "V" [VI]. 466: VIII 9-11). The *Grito de Dolores,* the signal for the Mexican revolution, had been uttered on 16 September 1810; and thenceforth the history of Mexico for many years was almost continuous warfare. In 1812 two new battalions of troops arrived from Spain; perhaps it was for these that the new quarters were built. Little notice had been given CERVANTES of the change in size of the Garden, and he found it necessary to move hundreds of plants without warning ("translacion repentina y tumultaria"). More than 600 plants were lost in the move, and much damage was done to the water supply. 412 pesos were allowed for repairs; but that did not bring back the lost glories of the Garden.

In 1813—the year of the Mexican declaration of independence—LOPEZ died, in the current epidemic of "fever" (AGH 466: unnumb. file 4). CERVANTES represented to the government that it was necessary to appoint a successor, who would understand the culture of plants, oversee the work of the gardeners and the collection of plants and seeds, and take care of the species both from Mexico and from the Old World which were cultivated in the Garden. He suggested, moreover, that some one could be obtained for 500 or 600 pesos annually, half what LOPEZ had received. He got no answer and no appointment was made for five years, during which time the Garden lost more than 300 "plantas curiosas y necesarias en las lecciones." The two Indians to which the horticultural staff had been reduced lived in idleness, not having any one to direct their work.

In the earlier years SESSÉ or CERVANTES had paid the bills from his own pocket, applying twice a year for reimbursement. Since the accounts might not be audited and settled for a year or so (those for the first half of 1791, for instance, were not paid until 12 June 1792; AGH 461: VI 6-8), CERVANTES suggested that a regular sum be advanced of 80 pesos a month; beginning in 1792 the Treasury proposed to advance him 500 pesos for each semester and this was apparently done from 1793 on (AGH 460: IV 179, 463: XIV). The year's expenses, until 1797, usually exceeded this sum, the highest being 1691 pesos 6 reales in 1793; usually it was between 1100 and 1200 pesos. CERVANTES at first attempted to differentiate between ordinary expenses and extraordinary, the latter covering tools, plants, and so forth which he imagined would not recur; this was the basis of his estimate for monthly payments. It was soon apparent that this distinction was of no significance.[97]

In 1810 the annual appropriation for the support of the Garden was reduced to 600 pesos (AGH 465: XIX, XXI). The expenses had actually fallen to between 700 and 800 pesos (AGH 466: III-V). Later they dropped sharply to about 500 pesos; area having been reduced and activi-

[97] The bills, with the attesting documents and receipts attached, were examined with meticulous care by the accountants of the Royal Treasury, and many were the items which they questioned or to which they raised objections. In spite of CERVANTES' attestation, many discrepancies were found between the summaries and totals for the semester and the detailed accounts of each month. For 1792, for example, there are in the archives two versions, each itemized, each with summaries attached. The bill for February in one place is 86 pesos 3 tomins 6 granos; in the other version the "86" becomes "68" (AGH 464: XIV). Other errors are caused by exclusion of certain items from one or the other version. Three totals appear, ranging from 1197 to 1123 pesos. How the *señores contadores* ever disentangled this mess, or how CERVANTES ever got reimbursed for the balance, I cannot see.

ties curtailed (AGH 465: XIX. 466: VI). The accounts became little more than notices of salaries paid and the expenses of opening the course and staging the *acto* at the end of it. The numerous items of former years, which testified to active growth and careful maintenance of the Garden, all disappeared.

By 1813 affairs had reached such a state that VELASCO was moved to report as follows (AGH 466: VIII 9-11). "Since the greater part of the Garden was taken for the barracks, it has been reduced to such a small extent that it does not allow that choice of plants which entitles it to rank as a botanical garden, nor can it provide the medicinal herbs most frequently necessary for the public. Further difficulty arises in the instruction of students, aside from the greater need of these persons in the present emergency to serve in the Battalions of Patriots or other bodies in defense of their country. And, to make a long story short, we are in the miserable situation of having exhausted the treasury, and of lacking resources for the maintenance of the armies which use up our substance and without which we and the kingdom should have perished." He went on to suggest that CERVANTES' original commission was for six years—the time allotted to the Botanical Expedition—and successors for both him and LOPEZ should now be found among the students. He proposed the promotion of DIEGO MARTIN, one of the gardeners, with a salary of 15 pesos monthly (a convenient reduction of five-sixths for this item!), and one assistant. He insinuated, furthermore, that CERVANTES had been for some time augmenting his income by the drugshop of the Hospital of San Andrés and in another in the Calle del Relox.[98]

In a burst of righteous indignation, CERVANTES surveyed the many economies already effected (AGH 466: VIII 13-18). It was not the first time [he said] that His Majesty's accountant had showed himself overzealous for economy at the expense of the Garden. Even NAPOLEON—"el barbaro Napoleon, desolador de Europa"—had respected such institutions, and the French had preserved the Royal Botanical Garden at Madrid. Only in Mexico was the extraordinary proposal made to deprive young men of the opportunity of learning botany. He calls attention to the lack of understanding displayed in the supposition that the purpose of the Garden was to provide the public with herbs. As for the small size of the Garden, in its present state it still contained more than 700 species of almost all the classes of plants. From 200 to 300 specimens were brought in daily from the surrounding country for the lessons—and these could not be supplied by even a large garden. From 16 to 20 students were still acquiring a knowledge of botany every year, whether or not they might be better employed in the fighting. The permanency of the Garden was obviously contemplated by the original regulations. As for his having derived support from drugshops, he said that indeed he could not otherwise have lived. But he gave his services to the hospitals, reformed their pharmacy, introduced Mexican plants to the pharmacopoeia, and so forth. "I have expounded from the Chair many new doctrines which the original plan of instruction did not prescribe; I have lived always loved and respected by my students (except the lazy and ignorant, who are the pests of all classrooms), and I have had the satisfaction of sending out from the course

[98] According to BERISTAIN (Bibliot. Hisp.-Am. 1: 336, 337), and LEÓN (Bibl. Bot. Mex. 88), he directed the *boticaria* of San Andrés for 18 years.

more well taught pupils than Velasco could imagine. . . ." DIEGO MARTIN was a poor Indian, unable to read, and lacking in the first elements of the art of gardening, capable only of cultivating a *chinampa* of peppers, tomatoes, garlic, and onions.

CERVANTES' letter evidently received the attention due it. The *Fiscal* (9 December 1813) recommended that no change be made in the Garden, beyond enforcing the utmost economy (AGH 466: VIII 34-36). He did try to appease VELASCO by suggesting that CERVANTES' salary be reduced to 500 pesos a year, and the staff limited to a single gardener. CERVANTES' very natural protest (4 February 1814) caused him to rescind this proposal also, and the *catedrático* continued to enjoy his already sufficiently meagre stipend (AGH 466: VIII 38, 45, 47). Somehow they seem also to have found means to continue to employ a pair of gardeners.

In 1817 CERVANTES complained that many plants were lacking which were needed for the lessons, a number having been lost "in these times," and the gardeners had not been able to go out into the country in search of new specimens because of the current disorders (AGH 466: unnumb. file 8). On several occasions they had been caught by the insurgents, who relieved them of their clothing and committed other outrages. At the moment things were quieter, and he was requesting a full passport for CARLOS ANTONIO to "recorrer las montes de Sn. Angel, San Agustn. de las Cuevas," and even to reach the *tierras calientes* of Quautla (Cuernavaca) and Yacapistla, as in former times.

In 1818 CERVANTES renewed his request for a successor to LOPEZ, naming JUAN LAZARI (or LASARY), who had charge of the garden of Don MANUEL FOLSA. He was appointed, at a salary of 500 pesos (AGH 466: unnumb. file 4, 5). In August the new gardener complained of the condition of his quarters, in which the floors were rotten because of the prevailing dampness. Official investigation disclosed that the place was indeed damp, the kitchen all the more so for being paved with brick; "in general," continued YGNACIO DEL VILLAR, "all the dwellings of this city suffer from this defect," (AGH 466: unnumb. file 1, 2). I have found no record of what (if anything) was done about it.

The records of the Botanical Garden in the *Archivo General* end with 1820, for the first half of which the appropriation dropped to 200 pesos. During the last years of the colonial government there was a brief Indian summer of peace and apparent security, during which the Viceroy could flatter himself that the revolutionary fires had died out. This delusion and the authority of Spain were ended together by the successful siege of the capital by Iturbide in 1821. In the ensuing confusion it is a matter for some surprise that the Botanical Garden apparently escaped damage. In 1823 it was visited by the English traveler WILLIAM BULLOCK, who devoted an entire chapter to it in his book.[99] "This beautiful establishment [he wrote] occupies one of the courts of the viceregal palace; and, though situated in the centre of a large and populous city, every vegetable production seems in perfect health and vigour." He wrote further of the "large stone basin in the centre, constantly supplied by a fountain with water, which, in small rivulets, spreads itself over every part of this little paradise." There is no mistaking the "tank" for which CERVANTES fought so long. It is perhaps

[99] Trav. Mex. Chapter 14. I am indebted to Dr. ROGERS MCVAUGH for calling my attention to this account.

well to remember that BULLOCK saw the Garden during the rainy season, when it was at its best. He saw the "celebrated hand-tree," many cacti, and "thousands of elegant plants and flowers, unknown to the eye of a European," seeds of 31 of which he sent to "Mr. Tate at the Botanic Garden in Sloane Street." Such enthusiastic exaggeration could scarcely have been prompted by a garden in the last stages of dissolution. He wrote, however, that the establishment was "about to be discontinued, the pension of the professor being stopped . . . . Even the trifling salary of the venerable little Italian curator was unpaid." We may assume that after 1820 CERVANTES managed to support himself by dispensing drugs in the hospitals and elsewhere; LEÓN's reference to a work by his son JULIAN (Bibl. Bot. Mex. 88) and the present existence of a collection of plants labeled with the same name[100] suggest that the courses in botany were continued by this individual, and were perhaps continuous with those given later by BENIGNO BUSTAMENTE Y SEPTIEM. The "venerable little Italian curator" must have been LAZARI, who evidently also continued at his place in spite of the discontinuance of support by the successive revolutionary governments. The story is one of gradual decay rather than of abrupt cessation of all activity. As late as 1843 (according to a note in Bot. Zeit. 1 : 855, 856. 8 D 1843), LAZARI was still the gardener; "now 110 years old, bent double, but still in possession of all his faculties." The garden was described as small but pretty, the trees of good stature and flowers luxuriant, but nevertheless a sad reminder of the past glories of science in Mexico.

CERVANTES died on 26 July 1829.[101] Of his private life and character little has been written; but some glimpses emerge from the correspondence in the *Archivo General*. There can be little doubt that he was a competent and successful teacher. He put the Botanical Garden and its course in botany in the first rank of the cultural institutions of New Spain; in those years it was the mark of an educated man to be versed in that science. Thirty years and more of successful teaching argue some scholastic soundness, to say the least; and such scattered papers as he published measure up well, in scientific worth, to the standard of the time. His life was not marred by that lust for administrative eminence, for dominance over the lives of others, which frustrates so much of man's well-intentioned planning. When he speaks of having lived always loved and respected by his students, there is no reason to doubt him. It is said that he was exempted from the general hatred with which the Spaniards were regarded during the revolutionary period (Bol. Soc. Mex. Geog. Estad. II. 1 : 765). The evident mildness of his manner was not, however, due to weakness. He could fight for his Garden when necessary and could stand up for what he thought right with the vigor born of his own unquestioned integrity.

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[100] In the Instituto de Biología are five cases of plants labeled "Don Julian Cervantes (1755-1829)." The son's name is here coupled with the father's dates. To my knowledge these specimens have never been studied. It is probable that they include specimens collected by SESSÉ and MOCIÑO as well as by CERVANTES himself. HUMBOLDT mentions CERVANTES' herbarium in his *Essai politique* (2 : 16) ; also his "riche collection de minéraux mexicaines."

[101] If we are to believe LEÓN (Bibl. Bot. Mex. 88) ; the age given by LEÓN is wrong, however, if his date is right.

Chapter VI

SESSÉ AND MOCIÑO: RETURN TO SPAIN

By 1799 the Expedition—save for LONGINOS MARTÍNEZ—was reunited in Mexico. It was time, and past time, for them to return to Spain. Their allotted six years with two years extension had expired in 1796. It is curious, therefore, that not until early in 1802 do we find any correspondence about leaving. It is true that there were large numbers of specimens to be worked over. The war with England undoubtedly made it difficult to obtain passage for Europe. One member of the Expedition was still missing. But all these difficulties scarcely explain the protracted delay in preparation for departure.

In 1801 everything was ready, or almost so; when a series of postponements was initiated in a manner that suggests the interference of a person or persons interested in keeping SESSÉ out of Spain. On August 7 the *Sala de Crimen* requested the Viceroy to deny SESSÉ permission to embark until he should satisfy a charge against him involving a sum of money lost at play by one Don JUAN MIGUEL YRIARTE.[102] The Viceroy pointed out the desirability of using all possible despatch, since the Expedition was living at government expense. Yet the next communication from the Criminal Court was dated a year and a half later. SESSÉ, meanwhile, had either not been informed of the restraint placed upon him or had not taken it seriously, for on 14 March 1802 he wrote at length to the Viceroy on the problems incident to the departure of the Expedition, for which he expressed himself as now ready. Ten days later, however, the court directed him to furnish power of attorney and to give bond for the satisfaction of the judgment and sentence which would be given, before leaving Mexico.

SESSÉ in a long letter to BERENGUER (March 27), pointed out that he was ignorant of the charge against him; moreover, this was a civil case and out of the jurisdiction of the Criminal Court; for which reasons the charge was clearly "ilegal, infundada y calumniosa." In fact (he said) the plaintiff should give bond for the cost of the delay in the departure of the Expedition; to say nothing of the serious consequences of delay in publication of his work. The affair dragged on, as legal affairs do. The *Fiscal* brought it to the attention of the new Viceroy, YTURRIGARAY; to whom he characterized SESSÉ's statement as "extravagant." In January 1803 the Viceroy intervened; but the court stood firm in its stipulation for "apoderado y fianza" as a condition of departure. In another long letter (28 January 1803) SESSÉ, who, having complied apparently with certain formalities, had thought himself free to leave, again advanced the question of legality. Of what use was it (he wrote) to say he could leave on two such impossible conditions as to give power of attorney without having seen the charges and to give bond in a criminal case contrary to law? "This is to place an enormous chain on each of my feet and then to tell me that I am free to run." He had (he said) nothing to fear and was not the man to run away; indeed the suit originated in Spain, whither he was about to return. The intent of the charges seemed to be to create an interminable case which

[102] The records of this business are found in AGH 465: XIII 1-23.

would indefinitely prevent his return to Spain and the completion of his mission, "so important to the nation and to all the human race," on which 150,000 pesos had been spent. To us the illegality of which SESSÉ complained seems obvious; but Spanish Law under the BOURBONS did not protect the subject against the government, or (as in this case) against his enemies who had political interest. The record is silent on the final disposition of the case. On February 28 the court was still standing pat; on March 5 SESSÉ was free to go, having apparently given in. For at least a year and perhaps two the Expedition had been held up in Mexico by this obviously trumped-up lawsuit.

The problems on which SESSÉ had written in 1802 concerned the personnel of the Expedition. One of the original members was dead, another was missing (he was dead too, or dying, though SESSÉ did not know it); MOCIÑO and several others had not come from Spain with the Expedition; CERVANTES had, but presumably should remain in Mexico to continue the work of instruction in the Botanical Garden. Such eventualities had not been foreseen in the royal orders which provided the authority for the return of the Expedition.

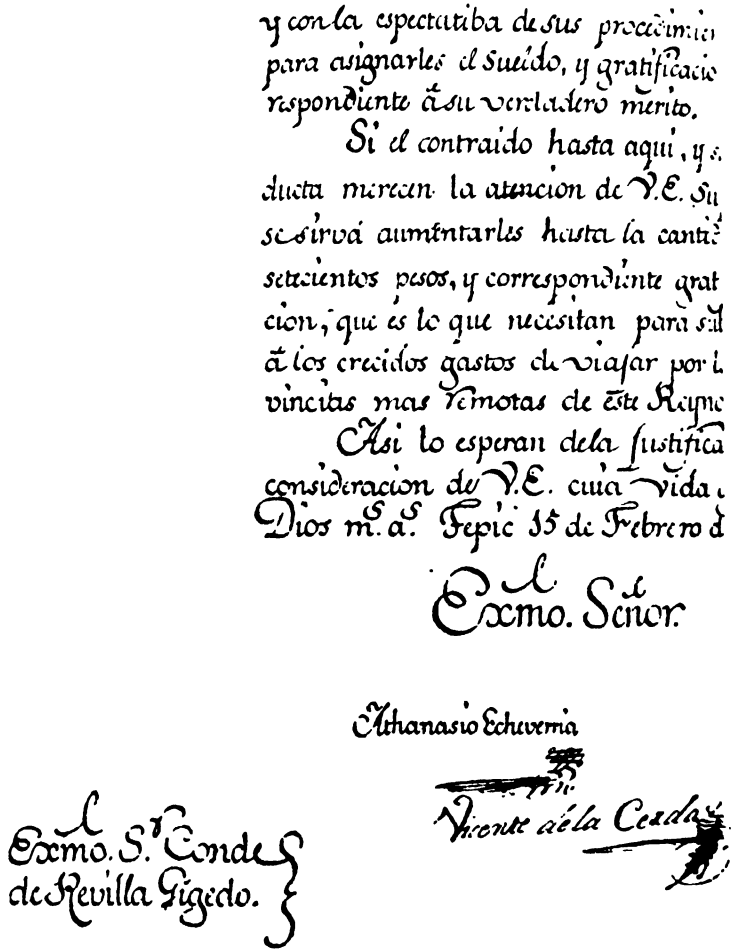
Much of SESSÉ's effort was directed towards enabling MOCIÑO to accompany the others to Spain, where he could help in the completion of the work in which he had already played so large a part. "Although he did not come from Spain [wrote SESSÉ], and the royal orders seem to apply only to those who did, there are weighty reasons for him to go with us. He has served 11 years in the Expedition in the place of one who would have returned if he had lived [SESSÉ stretches the truth a bit here, since CASTILLO died in 1793; MOCIÑO *first joined* the Expedition in 1790, 13 years before]. During this time he has made journeys to Nootka, to California, to Mizteca, and to Guatemala; consequently he is well qualified to conclude the work which he has begun by such vast excursions. Besides, I can truly say that I know of no other who would be capable of helping me to edit the various works which have been the object of our great commission. Finally, since he would enjoy in Mexico the same retirement salary as in Spain, since in the latter country he can help in the more prompt conclusion of my labors, and since he finds this occupation to his liking, there can be no good reason for opposing his desire, and indeed it would be most uneconomical to do so."[103]

As for the *pintores*, the royal instructions had provided that, in order

[103] AGH 465: XIV 1, 2. "Aunque el Botanico Dn. José Mariano Moziño, y el Dibujante Dn. Vicente de la Cerda no hayan venido de España, y las reales ordenes parezcan contraerse á solos los que vinieron de aquellos Reynos, hay poderosas razones para que vayan, como ellos apetecen. El primero ha servido once años en la expedicion la plaza de uno de los que vinieron de allà, y debia haver regresado, si no huviese perecido en ella. Durante este tiempo ha hecho por si solo, y de orden superior, las Expediciones de Nootka, de Californias, de las Mistecas, y del Reyno de Guatemala; por consiguiente es responsable, y ha de tener mas facilidad para concluir con acierto los trabajos qe. ha hecho en unas excursiones tan dilatadas. Fuera de que puedo decir sin mentira que no cuento con otro que sea capaz de auxiliarme en la edicion de las diferentes obras que han sido el objeto de nuestra vastisima comision. Finalmente, debiendo este gozar en Mexico el mismo retiro que en España, pudiendome alli ayudar á la mas pronta conclusion de todos los trabajos, y queriendo el seguir gustoso esta ocupacion, no encuentro una razon fundada, y si una gran falta de économia en oponerse á sus laudables deseos."

In this letter also SESSÉ took up the question of CERVANTES and LOPEZ, already detailed.

to keep up with the collections, they finish only one part of each species—one flower, one fruit, etc.—leaving the rest in India ink (*tinta de China*), to be colored later. As a result of this and of the pressure of the work they had more than 2000 partly finished sketches and about 400 in rough draft. Completion of these would be difficult for any other hands. He petitioned therefore that both artists accompany the Expedition to Spain;


y con la espectativa de sus procedimie
para asignarles el Sueldo, y gratificacio
respondiente á su verdadero merito.
Si el contraido hasta aqui, y s
ducta merecen la atencion de V.E. Su
se sirva aumentarles hasta la canti
setecientos pesos, y correspondiente grat
cion, que es lo que necesitan para sa
á los crecidos gastos de viajar por l
vincias mas remotas de este Reyno
Asi lo esperan de la justifica
consideracion de V.E. cuia vida
Dios m.s a.s Tepic 15 de Febrero d
Excmo. Señor.
Athanasio Echeverria
Vicente de la Cerda
Excmo. S.r Conde de Revilla Gigedo.

Conclusion of a petition from the artists of the Expedition for an increase of salary; written by ATHANASIO ECHEVERRÍA.

and that ECHEVERRÍA be ordered to rejoin it for that purpose.[104] The *Fiscal*, however, decided that LA CERDA should remain with the Botanical Garden

[104] SESSÉ pointed out that these accomplished artists had worked 13 years with the Expedition for a miserable salary—half what was paid to those of the Expedition to Peru; and retirement on half-pay would be a sorry end to all their work. In 1792 they had applied, under his auspices, for an increase. The records of this are interesting as providing autographs of ECHEVERRÍA, in which he demonstrated his talents by his beautiful penmanship (AGH 462: II). In those pre-typewriter days penmanship was still an art. He signed himself ATHANASIO ECHEVERRÍA Y GODOY. The ball was still rolling in 1795, but nothing ever came of it.

(16 Ja 1803; AGH 465: XIV 9). Of ECHEVERRÍA the archives make no further mention.

While MOCIÑO's fate was still unsettled, the redoubtable Doña MARÍA RITA RIVERA Y MELO MONTAÑO contributed her share of the current vexations and delays. The long-simmering suit for divorce now came to a head, with the associated demands on MOCIÑO's salary. "Since 1790 [wrote Doña MARÍA on 29 March 1803[105]] I have continued my representation for fulfillment of your decree of that year. In 1793 my husband promised me half his pay. In 1795, passing through Oaxaca on his way to Guatemala, he assured me of a written contract. None of these promises has been kept, except that I have had a third of his salary. Now I am afraid that he will go to Spain leaving me in poverty. . . ." MOCIÑO, in an undated letter, pointed out that the separation was voluntary on the part of his wife, and entered upon a long and complex argument on the "rights" (*derechos*) of matrimony. Unless (as is quite possible) he had SESSÉ's help in drafting this letter, it betrays a legalistic, not to say casuistic side of MOCIÑO's character not hitherto revealed; perhaps, however, not surprising in an erstwhile professor of philosophy and theology. He concludes by remarking that he had made the offer of a third of his salary without suspecting that she was systematically defaming him, and that under the circumstances he had the right to revoke his offer.

On the eve of departure SESSÉ wrote, "Doña MARÍA RITA RIVERA seems only to live in order to discredit her husband in every tribunal on earth." A series of letters on this paltry business need not weary the reader; they reiterate the fears and complaints of the lady (who refused to treat directly with her husband) that she would be left in Mexico with no guarantee of the continuance of her alimony. MOCIÑO was relieved of his embarrassment by a decision of the Treasurer General of 15 April 1803, directing that His Majesty be informed of the claim on his salary in Spain, as yet undetermined. In view of the rest of the story, any malicious hopes entertained by the reader to the detriment of Doña MARÍA's prospects are justified.

On 10 February 1803, MOCIÑO's destiny was still undetermined. On March 7, Doña MARÍA apparently believed that he was to sail on the following day. Finally a note from an official of the Treasury, dated 15 April 1803, mentioned that MOCIÑO was to go to Spain with the others, as ordered by His Excellency the Viceroy (AGH 465: XI 16, 18, 25). By this time he was probably already at sea.

Of the exact date of embarkation of the Expedition I have found no record. From March 15 to March 27 SESSÉ was in Jalapa (*i.e.*, already on the road to Veracruz) (AGH 465: XVII 3, 10); a note from the *Fiscal de lo Civil* dated March 29 speaks of him as having left the capital on his way to Spain (AGH 462: Ib 168). Evidently Doña MARÍA's suit was settled on the eve of departure, which must have been in April 1803. The party consisted of Don MARTIN DE SESSÉ, Director; Doña MARÍA GUADALUPE DE LOS MORALES, his wife; Don ALEXANDRO and Doña MARTINA, his children; Doña MARÍA JOSEFA DE LOS MORALES, his sister-in-law; Don JOSÉ MOCIÑO, botanist; Don JAYME SENSEVE, pharmacist; Don JOSÉ ANTONIO ZAMBRANO, clerk; AGUSTÍN BETANCUR, servant; and MARÍA YNES, negro slave of the Director (AGH 465: XV 1).

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[105] AGH 465: XI 32. The further records of this episode are on sheets 4-31.

SENSEVE, always impatient for the security of half-pay in Spain, went ahead to Veracruz, having instructions to take charge of the boxes, while SESSÉ, in case the vessel was not to touch at Havana, was to go to that port to pick up his collections in the West Indies. SENSEVE, characteristically, failed to sail because of incomplete arrangements for his maintenance ("defecto de ajuste de mesa"), and got into fresh trouble (for which, however, SESSÉ blamed the Governor of the port (AGH 465: XVII 3-10). From the list of the party given above it seems that they all finally sailed together; whether SESSÉ succeeded in picking up his Cuban and Puerto Rican collections I do not know.

These desultory records, these fragmentary leavings of a colonial bureaucracy, supply us with a miscellany of facts, official acts, and dates. Of values and personalities little is written on these dry and ancient sheets. Yet when they are collated and abstracted to yield a continued story, much emerges which illumines the characters and relationships of these men. When SESSÉ writes of MOCIÑO, whether it be the beginner of 1790 or the veteran collector of 1800, it is always with affection and admiration. He found in the young Mexican not only a capable and trustworthy but an evidently devoted helper. Of SESSÉ himself we know remarkably little. His letters reveal a clear and logical mind, a bent for system, joined with a conscientious devotion to the advancement of science, which must have fitted him admirably for the administration of a scientific expedition. He seems to have been successful in his dealings with all save LONGINOS MARTÍNEZ. As for MOCIÑO, from time to time he has peered out at us unmistakably through the veil of paper and ink, giving us a glimpse of a sufficiently lively young wayfarer, "gay and jovial," as his friend PABLO DE LA LLAVE said, exceptionally endowed with scholarly aptitude, loving knowledge and surprisingly stedfast in his pursuit of it to the detriment of his ease and prosperity. Whether his own feeling for SESSÉ was personal or a byproduct of his devotion to science I cannot clearly see; probably it was both, for SESSÉ certainly befriended him, undoubtedly made promises to him, and provided him with the means of following his bent. MOCIÑO later wrote that he came to Spain only to help in the preparation of the Flora of Mexico; and it is clear that both men were strongly influenced by the desire to achieve fame through the publication of a monumental work based on their collections and illustrated by the paintings of ECHEVERRÍA and LA CERDA—a work comparable to the *Flora peruviana et chilensis* of RUIZ and PAVON, which had appeared while they were exploring Mexico, and which they knew of, if they had not actually seen a copy. To the attainment of such fame present comforts might well be subordinated, and in the hope of it they confidently set sail for the Old World—a new world for one of them.

Like HERNÁNDEZ before them, SESSÉ and MOCIÑO were crushed by the official lack of interest in their achievements and plans. At this time over Spain, as over all Europe, hung the shadow of NAPOLEON. The treaties of 1800 and 1803 had reduced the Spanish monarchy to a subservient dependency upon the wishes of the imperial dictator. To the corrupt and self-seeking politicians kept in office by such a regime, nothing could be of less concern than a Flora of Mexico, and nothing more absurd than the diversion of large sums of money to its publication. MOCIÑO had to accept a meagre pension, and took refuge in SESSÉ's house, where he lived "like a pupil." SESSÉ, of course, had some means, and was not dependent

upon court favor for his support; but it is possible that his disappointment hastened his death, which occurred in 1809 or shortly before.

On Mociño's subsequent life I have nothing new to contribute. The story has been several times told, by Pablo de la Llave, by the anonymous biographer of the *Diccionario Universal*, by Miguel Colmeiro, by Augustin Pyramus de Candolle, by Francisco Sosa, by Ricardo Ramírez, and by Paul Carpenter Standley.[106] For the sake of continuity and in an attempt to resolve some of the inconsistencies in these accounts, I have undertaken to collate them here.

Poor as he was, Mociño did not lack honor in his new home. Cheated of further progress with the Flora, he resumed his interest in medicine, which had inspired his botany throughout his career. According to the *Diccionario universal*, in 1804 he was "Director de la policía médica" of Andalusia. We know from his own statement as well as from that of his friend La Llave that he labored courageously and unselfishly in a violent epidemic of yellow fever that devastated southern Spain at that time. He preferred to succor the poor rather than the rich—getting no pay for his services—and would go out to visit the sufferers at any time regardless of the rheumatism with which he was himself afflicted (Reg. Trim. 1: 346). Ramírez mentions a *Memoria* on yellow fever which was presented to the Royal Academy of Medicine in Madrid, but apparently never published. Mociño himself tells us that, since it contained opinions at variance with those of other physicians, it was submitted to the "junta superior de medicina," and by this committee in turn to a "censor anónimo," who disapproved it as he did most of the work of living authors that came to his hands. Mociño was twice secretary and four times president of the Academy, and to him, indeed, this body owed its continued existence. When the house which served it for meeting-place and headquarters was torn down, he moved its effects at his own expense to the "botanical office of the learned authors of the Flora of Peru and Chile." When this building in turn was to be destroyed, he moved the property of the Academy to the Convent of San Francisco. Throughout this period "his house was that of the Academy, without his receiving anything, in his great poverty, wherewith to buy fuel and so forth." Later he exerted himself to deny the presidency to "Mr. Parrois," Joseph Bonaparte's physician; such a dignity, he wrote, should not fall to a "profesor ignorante," who owed his position to his patron's caprice (Reg. Trim. 1 : 347, 348).

La Llave says that under the French occupation (1808-1812) Mociño was Director of the Museum (*gabinete*) of Natural History and gave its first courses in zoology. Of the several authorities cited for this part of his life, La Llave should be the most trustworthy, since he lived with Mociño and the two worked together in the arrangement of the zoological specimens of the Museum. The *Diccionario universal* bestows on Mociño the title of "Director of the Botanical Expedition of Mexico"—certainly an empty distinction after Sessé's death, if indeed it was maintained—and speaks of him also as a member of the "junta suprema de sanidad."

Sessé's widow returned to Mexico; where if she was seeking escape

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[106] Reg. Trim. 1: 44, 46, 47, 345-350. Colmeiro, Bot. Hisp.-Lusit. 185, 186. D.C. Mém. 219-221, 288-291. Naturaleza 7: 113-116. Sessé & Moc. Fl. Mex. viii, ix. Contr. U. S. Nat. Herb. 23: 15-17. Mention should also be made of the *Diccionario de geografía, historia y biografía mexicanas* by Leduc and Lara y Pardo (1910); the sketch of Mociño in this work is an extraordinary tissue of errors.

from political turmoil, she soon must have found that she had escaped from the comparative safety of the frying-pan into the hottest part of the fire. Her departure left Mociño homeless; it must have been at this time that he began to live in the quarters of the Academy of Medicine. Apparently all that stood between him and destitution was his position in the Museum. The French continued him in this work, even spoke of publishing the Flora, but evidently had no funds to do so. When they unexpectedly evacuated Madrid (in 1812), it apparently did not occur to Mociño that to have occupied an official post under French auspices would compromise him with the returning patriots. He soon discovered his mistake. With other distinguished men he was taken, on foot, from prison to prison, chained by the arm to the next in the line. Finally, at the entrance to Castille, a general set this illustrious chain-gang at liberty, and Mociño returned to his museum. When the French retreated for the second time, he accompanied them; this was a disorderly rout, in which even the rich (if they had French affiliations) had to walk. Mociño put in a cart the most precious possessions of the museum, his manuscripts and the illustrations for the Flora; at night he slept in the cart, by day he walked behind it. Ultimately the cart and its contents were taken over by a French general, only the manuscripts and certain of the figures being kept by Mociño. La Llave speaks of him at this time as old and sick (*anciano y enfermo*), and Standley writes that he "was now far advanced in years." Actually he was only 55; disappointment, poverty, and his disgraceful treatment by the Spanish had left their mark, and his health doubtless bore the imprint of years of difficult travel and rough living in the wildernesses of New Spain. Little remained of the "gay and jovial nature" which had charmed the friends of the young Mexican scientist.

He found asylum in Montpellier, where he lived for some time almost blind and in extreme want—eating beggars' bread. Finally he was recognized and succored by "certain French and German scientists." The story of his meeting with the great Augustin Pyramus de Candolle has been told by Standley. The French botanist in his *Mémoires et souvenirs* speaks of him as "Joseph Mocino, vieillard mexicain." The tale told by Standley implies that the name was well known to de Candolle; yet the latter writes that Mociño "had been charged by Charles VI, with Sessé, to make the Flora of Mexico"—which suggests scant familiarity with his actual history. Mociño does refer to a personal interview with the monarch Charles IV; some mention of this doubtless led to de Candolle's confused statement.

The greatest interest of the intercourse between Mociño and de Candolle attaches to the illustrations (*dibujos, dessins*) of the Mexican flora. Of these, some 1400 had reached Montpellier. Thanks to de Candolle's *Mémoires,* their story is now well known to botanists. De Candolle at first formed a plan of publishing a sort of Prodromus to the Mexican flora, using the figures and describing the new species. The text to accompany the figures had been lost on the way; de Candolle unearthed it from the cellars of the Tuileries, and found "three or four parcels half destroyed by damp," with nothing of value in them save some of the place-names. One wonders whether this was the original of the *Flora mexicana* or the *Plantae Novae Hispaniae!* The greatest shock to the biographer of Mociño, accustomed to the eulogies of Sessé and of others who knew him as a youth, is to read in de Candolle's pages that a glance through the famous illustrations revealed many errors in their names, and that he soon saw that

"the good old man had only the vaguest ideas of science"; he determined to continue the work alone, to "resolve this chaos." He gave up the idea altogether when the *Nova genera et species plantarum* of "H. B. K." began to appear; he did, however, base on these illustrations many new species described in his *Prodromus.*

It is probable that SESSÉ and MOCIÑO had indeed made mistakes in identification. They had only a few standard works, principally those of LINNAEUS, with which to attempt to classify a flora which was mostly new to science; they had no access to the large herbaria upon which these works were based. That they realized their limitations, indeed, is apparent from the second sentence of the preface to the *Plantae Novae Hispaniae.* Elsewhere in the same preface is evidence of their extreme conservatism in describing new species—this also leading them to misdetermination. It is true also that MOCIÑO's botanical experience had been chiefly that of a collector; his formal training, though probably sound, had been scanty; and he was out of touch with current developments in botany. Yet it is scarcely credible that he had "only the vaguest ideas" about the plants on which he had labored for more than twenty years, and we must discount DE CANDOLLE's judgment, allowing perhaps for his own impatience and certainly for MOCIÑO's failing eyesight and memory (attested by himself), his physical infirmity, and for the long interval during which he had been engaged with other matters.

About 1816 MOCIÑO resolved to return to Spain. He addressed a long letter to the Academy of Medicine, asking that "respetable cuerpo," in view of the distinguished services which he briefly recounts, to intercede for him with the king to provide him with the means of a return to Spain "with honor."[107] Whether or not this letter found its mark, he was invited to return by his intimate friend Don JUAN JABAT, Minister of Marine; more probably as a result of the revolution of 1820, which brought back MOCIÑO's friends into power. JABAT offered him his own home until other means could be found for him.

MOCIÑO had written to DE CANDOLLE for his figures, the "property of the king," which he had confided to his care. In his *Mémoires* DE CANDOLLE tells how 1200 of this wonderful collection were copied in ten days by about 120 persons in Geneva. He then took the originals to Montpellier and put them in MOCIÑO's hands. He parted from him with regret and fear; an old man, sick and nearly blind—with all that remained of the great scientific expedition sent out by the King of Spain.

There is little more to tell. MOCIÑO disembarked in Barcelona, and there lodged in the house of Don JACOBO VILLA URRUTIA. Here he shortly suffered a hemorrhage and died. He was buried on 19 May 1820, in the Parochial Church of St. James of Barcelona.[108]

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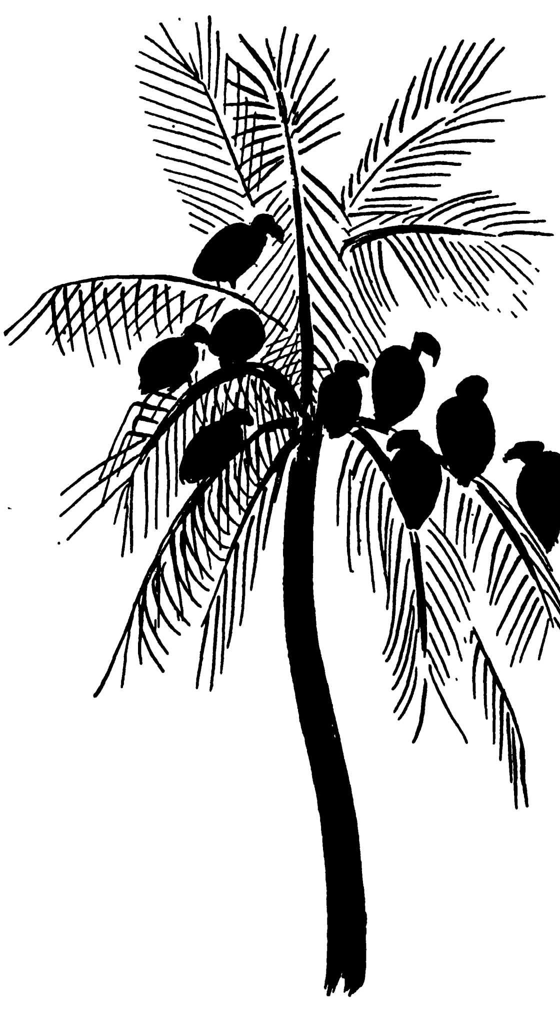
[107] Letter reproduced by LA LLAVE; Reg. Trim. 1: 348-350.

[108] The date of his death has been variously given as 1819, 1820, and 1822; most authorities, including LEÓN, CARREÑO, and STANDLEY, have followed COLMEIRO in making it 1819. The only thing in favor of this date is DE CANDOLLE's account of the copying of the illustrations and the return of the originals to MOCIÑO. This occurred in 1817. The assumption is made that MOCIÑO actually returned to Spain in that year. On the other side we have the undoubted fact that the regime which was unfavorable to MOCIÑO was not ousted until 1820; we have LA LLAVE's nearly contemporary account; and we have the burial certificate, of which a photographic copy is preserved in the library of the New York Botanical Garden. It is quite possible that MOCIÑO's hopes of returning to Spain in 1817 were based upon misinformation;

and that he did not receive the intercession by the Academy of Medicine upon which he was counting. It is much to be regretted that La Llave did not record the date of the letter to the Academy; in it the writer says that he had not left Spain for love of the French, and could not return to Spain for lack of means. This, if it was written in 1816 or 1817, suggests that he was looking for help in getting back to Spain, and had some hopes of finding some; but there is no evidence that he did actually return before 1820.

The burial certificate reads as follows: "Certifico, que en uno de los libros de defunciones de esta parroquin se halla una partida en catalan que vertida literalmente en castillano dice asi: a los diez y nueve de Mayo de mil ochocientos veinte en la Parroquial Yglesia de San Jaime de Barcelona, se hizo la deposicion del cadaver de D. José Moziño, Médico, viudo, natural de Oajaca en America, de edad sesenta y dos años, fallecido de un flujo de sangre recibiendo los sacramentos de la Penitencia y Extrema uncion. Fue concierto de seis hachas—calle de San Honorato." The signature of the clerk follows, and the original Catalan version.

It will be noticed that the document contains several errors of fact, including Mociño's age; he was 63 in 1820. This, of course, detracts from its conclusiveness. It is also possible, though very improbable, that burial had been delayed.



Appendix 1

PLANTS INTRODUCED INTO THE ROYAL BOTANICAL GARDEN OF MEXICO

The names below are found in the itemized accounts of the Garden. Besides these, there are many entries of "diversas plantas" received. The introduction of plants continued at least until 1806.

Conjectures of the identity of the species are based mainly on MARTÍNEZ' *Catálogo alfabético.*

Adelfa doble (*Nerium oleander* L.); 1793.
Aguacate (*Persea americana* Mill.); 1791.
Alelios (*Viola* or *Matthiola?*); 1794.
Azucenas blancas (*Lilium candidum* L.); 1795.
Botoncillo (*Centaurea cyanus* L.); 1792.
Cacaloxochitl (*Plumeria* sp.); 1791.
Castaños (*Castanea sativa* Mill.); 1791.
Chabacano (*Prunus armeniaca* L.); 1791, 1792, 1793.
Claveles (*Dianthus* sp.); 1794.
Clavellinas (*Dianthus caryophyllus* L.); 1792, 1799.
Clavillos (*Choisya ternata* H.B.K.); 1799.
Cleome (*Cleome*); 1795.
Cocolmeca (*Dioscorea* or *Piper* sp.); 1799.
Cruces de Jerusalem (*Lychnis chalcedonica* L.); 1794.
Croton (*Codiaeum* sp.); 1793.
Floripundios (*Datura arborea* L.); 1791.
Fresa (*Fragaria* sp.); 1805.
Guindos (*Prunus cerasus* L.); 1792.
Higuera (*Ficus carica* L.); 1792.
Hucle de noche (name applied to several sp. of Rubiaceae, Solanaceae, etc.); 1794.
Junquillos (*Narcissus*); 1794.
Mercadela (*Calendula officinalis* L.); 1792.
Naranjos (*Citrus*); 1792.
Nardos (? *Nardostachys jatamansi* DC.); 1794.
Nogales (*Juglans*); 1791.
Nopalillo (Cactaceae); 1793.
Palmas (Palmaceae); 1794.
Perales (*Pyrus communis* L.); 1791, 1792.
Rosales de Castilla (*Rosa* ? *centifolia* L.); 1791, 1792.
Rosales ordinarios (*Rosa*); 1791.
Tomillo (*Thymus vulgaris* L.); 1801.
Yedra (? *Ipomoea* sp.); 1792.
Yerba llamada de Sta. Alaria; 1793.
Yoloxochitl (*Talauma mexicana*); 1793.
Zapote (*Manilkara zapotilla* Gilly, or *Calocarpon mammosum* Pierre); 1791.
Zapote blanco (*Lucuma hypoglauca* St., or *Casimiroa* sp.); 1791.

Appendix 2

NAMES AND DATES OF CERTAIN GRADUATES OF THE COURSE IN BOTANY OF THE ROYAL BOTANICAL GARDEN

The exercises for the years 1788, 1789, 1792-1794 were published as pamphlets by ZUÑIGA in Mexico and are noticed by COLMEIRO (Bot. Hisp.-Lusit. 12, 13) and by LEÓN (Bibl. Bot. Mex. 86). Those of 1788 were described in Gaz. Mex. 3: 204 (23 D 1788) and 213 (6 Ja 1789); those of 1789 in Gaz. Mex. 3: 439 (22 D 1789). The names for 1798 are those of recipients of prizes of 50 pesos each, duly noticed in the accounts of expenses for that year and recorded in AGH 463: XIII.

1788. JOSEPH VICENTE DE LA PEÑA.
FRANCISCO GILES DE ARELLANO.
JOSEPH TIMOTEO ARSINAS.
1789. JOSEPH MOCIÑO.
JUSTO PASTOR Y TORRES.
JOSEPH MALDONADO.
[JOSEPH MIGUEL PERDIGON.
LUIS RODRIGUEZ.]*
1792. PEDRO MUÑOZ.
SEBASTIAN GOMEZ MORON.
MANUEL MARÍA BERNAL.
FRANCISCO PERALTA.
1793. JOSEPH AGUSTÍN MONROY.
PEDRO REGALADO TAMES.
IGNACIO FERNANDEZ DE CÓRDOBA.
1794. JOSEPH FERNANDEZ VARELA.
JOSEPH DIONISIO LARREATEGUI.
IGNACIO LEÓN Y PEREZ.
1798. YGNACIO NAVAMUEL.
FULGENCIO ARANJO.
RAMON COVARRUVIAS.
ANTONIO CESPEDES.

* Mentioned in the Gazeta but not in COLMEIRO's and LEÓN's citations.

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Beristain de Souza, José Mariano. Biblioteca hispano-americana septentrional, ó catálogo y noticia de los literatos, que ó nacidos, ó educados, ó florecientes en la América septentrional española, han dado á luz algun escrito, ó lo han dexado preparado para la prensa. 1: I-XVIII, 1-540. 1816; 2: 1-525. 1819; 3: 1-366. 1821.

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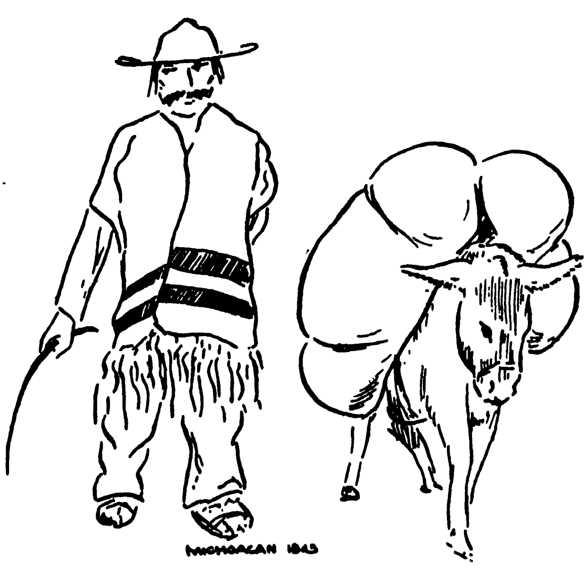

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CHRONICA BOTANICA, An International
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• Volume 11, Number 2, Pages 87-118, Plates 53-56 •
(Issued Autumn 1947)

FONTES HISTORIAE BOTANICAE ROSSICAE

Page from an early (17*th* century) Russian manuscript herbal chiefly based on SYRENIUSZ SYREŃSKI's Polish Herbal.

FONTES HISTORIAE BOTANICAE ROSSICAE

by

VLADIMIR C. ASMOUS, B.A.

Harvard College Library,
formerly, Assistant Librarian, Arnold Arboretum

— PRINTED IN THE UNITED STATES OF AMERICA —

PREFACE

In the following account an endeavour has been made to enumerate all publications which deal, in some way, with the history of botany in Russia. Not only phytohistorical publications *s.s.*, but also biographical and bibliographical publications have been included. Some special attention has even been given to general bibliographical works as they are often very useful for the student of Russian botany.

Such well-known bibliographical sources of a *general* nature as PRITZEL's *Thesaurus*, the unsurpassed *Bradley Bibliography* (which has a special Russian index), the *British Museum Catalogue*, etc., which will all be listed, in due time, in VERDOORN's "Bibliography of Plant Science Bibliographies", have been omitted, though they contain many papers concerned with Russian botany. On the other hand many minor, purely Russian bibliographies have been included because they are little known abroad, though helpful to the botanical historian, and often not so rare in the larger libraries of the world as one might suspect.

The titles of papers published in a language which uses the Latin alphabet are usually given in the language in which they were written. Titles in Russian of papers published in periodicals have been translated into English, such articles being indicated as "in Russian" in the annotations. However, if a Russian article has a French or Germany summary or title, it has been listed in the language of the summary and noted as "in Russian with a French (or German) résumé" in the annotations.

Individual, separate publications in Russian have been listed both under the transliterated and English titles. In transliterating the titles, I strictly observed the Library of Congress rules, except for the names of those authors who, themselves, consistently followed a different transliteration (*e.g.*, "FEDTSCHENKO" although, *sec.* the Lib. of Congress Rules, it should be "FEDCHENKO").

Each article included has been carefully examined. Such titles as were not available have been marked "not seen".

I overlooked, without doubt, some valuable titles; but I hope that my enumeration will be useful to those interested in the history of Russian Botany.

The illustrations have been selected by the Editors of CHRONICA BOTANICA from their collections and from materials in the Arnold Arboretum Library, in consultation with me. The legends have mostly been taken from Dr. VERDOORN's unpublished notes on the cultural history of botanical gardens.

When I graduated from the Military Engineering College at St. Petersburg in 1912 I hardly dreamed that, some day, I would be a bibliographer. The world of books and letters, in which I now live helps, as JEREMY COLLIER said: "to forget the crossness of men and things, it composes our cares and lays our disappointments asleep."

I should like to thank Dr. E. D. MERRILL who introduced me to bibliographical botanical studies and gave me the opportunity to work, for sixteen years, under his inspiring, friendly and stimulating guidance, on various important bibliographical projects, in the libraries of the New York Botanical Garden and the Arnold Arboretum. I am also obliged to Dr. F. VERDOORN, at whose request this bibliography (which will be followed by a biographical dictionary of Russian botanists of the past) was undertaken.

V. C. A.

The Russians have been lovers of nature, flowers and plants since olden times. The earliest visitors were amazed to note the nice flowering pot plants found in the homes of people of moderate means.

An American reporter, who visited Siberia in the late 1880's, was quite surprised (Century Mag., May 1889) to find many of the better educated exiles building up herbaria of local plants ("one of the few industries or amusements with which they are permitted to console themselves").

In the early eighteenth century, PETER THE GREAT, who had a small herbarium of his own, began making a fine garden at the Admiralty Island in addition to the Apothecaries Garden (p. 114 and PLATE 55). A little later Peterhof (p. 115), opposite St. Petersburg, under the direction of LE BLOND, became a Russian counterpart of the French Court Gardens. In addition to the cascades and terraces, in the French style, the garden was famous for a large number of importations from abroad. Many other gardens were now being developed by the Russian nobility of which those of Prince DEMIDOFF near Moscow were particularly renowned. P. S. PALLAS issued a catalogue of these gardens in 1781, with a plan which we reproduce on PLATE 54 (Latin legend of the plan, bottom left).

The Imperial Botanic Gardens of St. Petersburg (PLATE 55) developed greatly in the 19*th* century, towards 1900 certain of its collections and holdings were unsurpassed anywhere in the world. The Gardens continue still today but will be superseded gradually as *the* National Botanical Gardens by the new Gardens being laid out in Moscow (CHRON. 5: 492 *et seq.*). According to a recent report from one of our associates, the Academy of Sciences of the USSR has entrusted N. TSITSIN with the development of the new Garden in Moscow which will be the largest in the Union. An area of about one thousand acres has been set aside and the chief objective will be the study of the best ways and means of utilizing the country's flora for the nation's economy.

Much attention also will be paid to tree planting, cultivation of decorative plants and the creation of parks and gardens in the rehabilitated towns.

"Not only the soviet flora but also plants from all over the world will be represented in the gardens," it is stated. "The agricultural plants, from their wild ancestors to the present descendants, will be presented in a way reflecting most vividly DARWIN's evolutionary theory of the origin and development of plants, particularly as a result of man's labour."

The plan further provides for the construction of laboratories, museums, a herbarium, libraries and lecture halls and extensive experimental plots. The origin and evolution of cultivated plants, as potatoes, cotton, flax, strawberry, and fruit trees and the physiological and biochemical processes in plants, "particularly the texture, formation and disintegration of the structural albumens and ferments" are being studied. Interesting experiments are said to be made on "rejuvenating plants by surgical methods."

During the recent siege of Leningrad things were difficult and much damage was done. In a remarkable report ("Botanical work carried out in Leningrad in the course of the three years of the War, 1941/43", published in 1946, from which the vignette above, showing the main building of the Komarov Institute, has been taken), our Russian colleagues tell how they managed to continue in spite of the terrors of the Second World War. During the shelling of Leningrad about eighty shells fell in the Botanic Garden. Valuable trees were damaged and the large conservatory, with its tropical and subtropical plants, was destroyed. More than twenty hothouses have been rebuilt since then. As a result of the heroism of the staff of the Garden which remained in Leningrad, the Herbarium and the Botanical Library were saved. Work is now again well under way on the "Flora of the U S S R." and other publications, as the compilation of vegetation maps of various regions. Medicinal herbs were reintroduced into cultivation during the siege of Leningrad, and *Digitalis* and *Belladonna* of good quality were grown (supplies of which were even sent to Moscow). The staff of the Garden continues to assist in the development of vegetable gardens. During recent years they grew millions of seedlings of vegetables. Experiments are being carried out to induce biennials to bear fruit the first year in order to obtain large quantities of seeds of beets, cabbages, etc. By blacking-out hothouses for some hours and thus artificially creating short-day conditions, they were very successful.

Of other recent reviews we may still mention "The Symposium on Advances in Biological Sciences in the USSR within the recent 25 years (1917-1942)", published by the Russian Acad. of Sciences (pp. 355, 1945), and edited by L. A. ORBELI, which contains several botanical chapters: A. N. KRYSTOFOVICH, Paleobotany; B. K. SCHISCHKIN, Plant systematics and floristics; E. M. LAVRENKO, Soviet geo-botany (phytocoenology); N. A. MAXIMOV, Development and attainments of plant physiology; and, B. L. ISACHENKO, Development of microbiology.

Frontispiece of SOBOLEWSKI's *Flora Petropolitana* (1799), showing Flora offering her gifts to the Imperial Authority. The author (1741-1807) of this early Russian flora, who had studied at Paris and Leyden, lectured at the University and was temporarily in charge of the St. Petersburg Gardens. He dedicated his book to 'Paulo Primo, Autocratori Moderatissimo' (the son of CATHERINE THE GREAT). The words at the bottom (Tvoia ot tvoikh) are well known to all familiar with the liturgy of the Russian Church.

Akademiia Nauk [**Academy of Sciences**]

— — 1818-25: Polnoe sobranie uchenykh puteshestviĭ po Rossii, izdavaemoe Imperatorskoĭ Akademieiu Nauk. [A complete description of the scientific expeditions to Russia, edited by the Academy of Sciences]. 1: i-xxxiv, 1-493. 1818; 2: i-x, 1-486. 1819; 3: i-ii, i-viii, 1-540. 1821; 4: i-ii, 1-436. 1822; 5: i-ii, 1-492. 1822; 6; i-xii, 1-546. 1824; 7: i-vi, i-ii, 1-223. 1825. — *A history of early botanical expeditions; vol. 1-2 by* **S. Krasheninnikov**; *vol. 3-5 — by* **I. I. Lepekhin**; *and vol. 6-7 — by* **J. P. Falck.**

— — 1872-82: Tableau général méthodique et alphabétique des matières contenues dans les publications de l'Académie Impériale des Sciences de St.-Pétersbourg depuis sa fondation. 1-re partie. Publications en langues étrangères. i-xii, 1-488. 1872. Supplément 1, depuis 1871 jusqu'au 1 novembre 1881. i-vii, 1-56. 1882. — *A subject bibliography in French including a section "Botanique," pp. 196-217 and "Liste des présidents et des membres de l'Académie depuis sa fondation," pp. 407-488.*

— — 1875: Sistematicheskiĭ i alfavitnyĭ ukazatel' stateĭ, pomieshchennykh v periodicheskikh izdaniiakh i sbornikakh Imperatorskoĭ Akademii Nauk, a takzhe sochineniĭ, izdannykh Akademieiu otdiel'no, so vremeni eia osnovaniia po 1872 vkliuchitel'no. Chast' II. Sochineniia na russkom iazykie. [Systematical and alphabetical index of the articles published in periodical and non-periodical publications of the Imp. Academy of Sciences, and also separate works published by the Academy from its foundation to the year 1872 inclusive. Part II. Publications in Russian.] i-x, 1-402. — *A classified bibliography, including botanical works, pp. 105-109; see also sections "agriculture—forestry," pp. 86-92, "travels," pp. 133-151,* etc.

— — 1885-1900. Materialy dlia istorii Imperatorskoĭ Akademii Nauk (1716-1750). [Materials on the history of the Imp. Academy of Sciences.]. 10 vols. — *Includes documents, correspondence, portraits and other historical materials, some concerning botanists, members of the Academy.*

— — 1904-17: Russkaia bibliografiia po estestvoznaniiu i matematikie, sostavlennaia sostoiashchim pri Imp. Akademii Nauk S.-Peterburgskim biuro mezhdunarodnoĭ bibliografii. [Russian bibliography of natural history and mathematics, compiled by the Bureau of International Bibliography of the Imp. Academy of Sciences.] Vol. 1 (for 1901) — 9 (for 1912-13). *A very good classified bibliography, including section "M. Botanika" and "K-III. Paleofitologiia."*

— — 1912-16: Catalogue des livres publiés par l'Académie Impér. des Sciences. Partie I. Publications périodiques, recueils et séries en langues russe et étrangères depuis 1726 jusqu'au 1 juin 1912. [1-6], 1-148. 1912; Partie II. Ouvrages publiés séparément en langue russe depuis 1726 jusqu'au 1 juin 1915. i-ii, 1-184. 1915; Partie III. Ouvrages publiés séparément en langues étrangères depuis 1726 jusqu'au 1 mars 1916. i-iv, 1-162. 1916. — *Includes many works on the botany of Russia.*

— — 1915-17: Imperatorskaia Akademiia Nauk. [Imperial Academy of Sciences] 1889-1914. Vol. 2. Materialy dlia istorii Akademicheskikh uchrezhdeniĭ za 1889-1914 gg. [Materials on the history of the institutions of the Academy of Sciences for the years 1889-1914.] Pt. 1: i-iii, 1-631. 1917; Vol. 3: Materialy dlia biograficheskago slovaria dieĭstvitel'nykh chlenov Imper. Akademii Nauk. [Materials for a biographical dictionary of the active members of the Imper. Academy of Sciences]. Pt. 1 (A-L): i-iv, 1-440, 1915; Pt. 2 (M-Ia): i-iv, 1-335, 1917. — *Vol. 2 includes an article "Botanical Museum," pp. 133-160,*

describing the progress of that institution and vol. 3 contains some biographies of botanists and bibliographies of their writings.

——1927: Akademiia Nauk Soiuza Sovetskikh Sotsialisticheskikh Respublik za desiat' let, 1917-1927. [Academy of Sciences of USSR for ten years 1917-1927.]. [1-3], i-iii, 1-235.—*Includes sections: "botany," pp. 46-50, "plant physiology," pp. 51-56, etc.*

——1936: Katalog imeiushchikhsia v prodazhe izdaniĭ Akademii Nauk, 1769-1935. Knigi na russkom iazyke. [Catalogue of the publications of the Academy of Sciences from 1769 to 1935 available for sale. Books in Russian language.] i-viii, 1-240.—*A classified catalogue of individual authors, including sections; botany, paleontology, genetics, forestry, etc.; compiled by* **L. N. Pliushkin & E. K. Geidrich.**

——: Ocherk istorii muzeev Imperatorskoĭ Akademii Nauk. [Survey of the history of muzeums of the Academy of Sciences]. 1-88.—*Includes historical data on Botanical Museum; not seen.*

Alekhin, V. V., 1940: Floristics and plant systematics, geo-botany and phytocoenology at Moscow University. (Uchen. Zapisk. Mosk. Gosud. Univ. 54 (Biol.): 269-287. *pl.* (*port.*)).—*Historical.*

Alisova, E. N. (Alisova-Klobukova, E. N.) 1923: Botanical explorations in Primorsk region. (Primorie (Prir.) 117-122, text map.)—*A historical account.*

Andreevsky, I. E., *see* **Brockhaus, F. A. & Efron, I. A.**

Anonymous, 1914: Bibliography [1912-1914]. (Bull. Jard. Bot. Pierre Grand 14: 521-593.).—*Although this subject bibliography of more than 1000 entries includes some papers concerning other countries, it deals chiefly with botanical works on Russia.*

Arseniev, K. K., *see* **Brockhaus, F. A. & Efron, I. A.**

Bacmeister, J., 1766: Essai sur la bibliothèque et le cabinet de curiosités et d'histoire naturelle de l'Académie de Sciences de St. Pétersbourg. 1-254. (German translation—*1777:* 144 p.).—*Includes data on early botanical collections.*

Bagalieĭ, D. I., 1893-1904: Opyt istorii Khar'kovskago Universiteta. [Essay on the history of Kharkov University]. 1 (1802-1815): i-ii, 1-1204. *7 port.* 1893-98; 2(1815-1835): i-iv, 1-1136. *7 pl.* 1904.—*A comprehensive historical work including data on Botanical department, botanical museum and garden (1: 459-474: 2: 472-473) and biographical and bibliogrophical materials. (1: 435-778; 2: 436-849).*

Balabaev, G. A., 1930: Nature, agriculture and the future outlook of plant industry in Yakutia. (Bull. Appl. Bot. & Pl. Breed. 23(5): 407-511. *map. tables 1-10*).—*In Russian; includes survey of the region and a bibliography of 151 entries.*

Baliński, M., 1862: Dawna Akademja Wileńska. (Ancienne Académie de Wilno). i-xi, 1-606.—*A history of the Academy in Polish from 1579 to 1803, including some data on botanical activity.*

Ballion E., 1882: Table générale et systématique des matières contenues dans les premiers 56 volumes (années 1829-1881) du Bulletin de la Société Impériale des naturalistes de Moscou. 1-110.—*A classified index, including sections "Botanique," pp. 37-53, "Voyages," pp. 96-99, and "Mélanges," pp. 99-108; for the second part see* **"Société des naturalistes de Moscou."**

Baranov, V. I., 1927: The vegetation of the chernozeme zone of Western Siberia, (Zapisk. Zap.-Sibir. Otd. Russk. Geogr. Obshch. 1927: 1-160. *6 pl. map.*).—*In Russian with an English résumé; includes a list of Russian and foreign literature.*

Batalin, A., 1881: Aperçu des travaux russe sur la géographie des plantes, de 1875-1880. 1-25.—*A critical review; at head of title: "3. Congrès international de géog. Société Impériale russe de géographie."*

Beise, T., 1852: Die Kaiserliche Universität Dorpat während der ersten fünfzig Jahre ihres Bestehens und Wirkens. Denkschrift zum Jubelfeste am 12 und 13 December 1852. 1-168.—*A historical survey including valuable biographical and bibliographical data. See* **Recke, J. F. von & Napiersky, C. E.,** and **Khotinskiĭ, M.**

Beketov, A. N., 1853: Ocherk Tiflisskoĭ flory, s opisaniem liutikovykh eĭ prinadlezhashchikh. [An account of the Tiflis flora, with a description of the Ranunculaceae belonging to it]. 1-56. *3 pl. 1 map.*—*Includes data on botanical exploration with some bibliographical notes.*

——**& Gobi, C.**, editors, 1886-92: Bibliographie. (Script. Bot. Hort. Imp. Univ. Petrop. 1: 215-232. 1886; 349-410. 1887; 2: 1-72. 1887; 73-108. 1888. 109-152. 1889; 3: 469-542. 1892.).—*A bibliography of current literature on botany of Russia with critical reviews of some works.*

—— **& others,** 1893: XXV. Obzor dieiatel 'nosti S.-Peterburgskago Obshchestva Estesvoispytateleĭ za pervoe dvadtsatipiatilietie ego sushchestvovaniia. [Survey of the activity of the Society of Naturalists of St.-Petersburg for the first 25 years of its existence]. 1868-1893. i-viii, 1-228. Suppl. cxxvi. *port. 2 maps.*—*Includes "Activity of the Section of Botany", pp. 28-99, by* **M. S. Voronin & N. I. Kuznetsov** *and lists of communications, pp. lxvii-lxxxix, and papers, pp. xc-xcv, published by the members in the "Trudy" [Transactions] of the Society.*

Beliaev, O., 1800: Kabinet Petra Velikago. [Cabinet of Peter the Great]. 1: 1-215. 1800; 2: 1-287. 1800; 3: 1-278. 1800.—*Includes data on early botanical collections.*

Berg, E., Herder, F. von & Klinge, J. C., 1852-1899: Catalogus systematicus bibliothecae horti imperialis botanici Petropolitani. i-xvi, 1-515. 1852; editio nova. i-xi, 1-510. 1886;...(1886-1898) i-vii, 1-253, 1899;...(1898) 1-47. 1899. —*Original edition by* **E. Berg**; *ed. nova by* **F. von Herder** *and the rest by* **J. Klinge**; *includes many works on botany of Russia.*

Berg, L. S., 1925: The rôle of the Academy of Sciences in the history of geographical discoveries. (18 century). (Priroda 1925(7-9): 143-159. *port. map.*).—*A historical sketch in Russian, including brief biographies and data on botanical activities of many prominent botanists and explorers.*

—— 1926: Russian discoveries in the Pacific. (The Pacific Russian scientific investigations. (Published by the Academy of Sciences USSR), 1-26. *2 f. port. 2 maps.*—*In Russian. Historical notes on various voyages, including some botanical expeditions.*

—— 1945: The Geographical Society centenary (1845-1945). (Priroda 1945 (6): 78-88. *Ports.*).—*In Russian; a historical sketch of the R. G. S. with brief biographies of some botanists and explorers and data on their expeditions*).

Bibliograficheskiĭ ezhegodnik. Sistematicheskiĭ ukazatel' literatury za...god. [Bibliographical yearbook: a systematical guide of the literature for the year...]. 1912-27: Fas. 1 (1911)—8 (1924).—*Lists of new literature arranged according to the decimal system; includes botanical works. Edited by* **I. V. Vladislavlev**; *for continuation see* **'Ezhegodnik gosudarstvennoĭ tsentral'noĭ knizhnoĭ palaty.'**

Bibliothèque Impériale publique de St.-Pétersbourg, 1873: Catalogue de la section des Russica ou écrits sur la Russie en langues étrangères. Tome I (A-M): i-viii, 1-845. 1873; Tome II (N-Z). Supplément. Table méthodique. 1-772. 1873.—*An alphabetical list including several botanical works.*

Bieliński, J., 1899-1900: Uniwersytet Wileński (1579-1831) (L'Université de Wilno). 1: 1-485. 1899-1900; 2: 1-845. 1899-1900; 3: 1-734. 1900.—*A historical sketch in Polish; volume 2 includes sections: "Botanika", pp. 133-143, and "Agronomia", pp. 154-169, with bibliographies of works and biographical data.*

—— 1907-13: Królewski Uniwersytet Warszawski (1816-1831). 1: i-viii, 1-767. *illus.* 1907; 2: i-viii, 1-877. 1911; 3: i-vi, 1-755, 1-49. 1912-13.—*A history in Polish, including data on the Botanical section, vol. 3: 242-261, with a few brief biographies of botanists and bibliography of their works.*

Biograficheskiĭ slovar' professorov... Iur'evskago Universiteta, *see* **Levitsky, G. B.**

——...Kazanskago —— **Zagoskin, N. P.**

Biograficheskiĭ slovar' professorov i prepodavateleĭ Imp. Moskovskago Universiteta 1755-1855. [Biographical dictionary of professors and instructors of the Imp. Moscow University...]. 1755-1855. Pt. 1: i-xx, [1-6], 1-485. 1855; pt. 2: [1-9], 1-673. 1855.—*Includes biographies of some botanists and bibliographies of their works.*

Biograficheskiĭ slovar' professorov i prepodavateleĭ Imp. S. Peterburgskago Universiteta za istekshuiu tret'iu chetvert' vieka ego sushchestvovaniia. 1869-1894. [Biographical dictionary of professors and instructors of the Imp. St.-Petersburg University for the third quarter of the century of its existence. 1869-1894.]. 1 (A-L): 1-419. 1896; 2 (M-Ia): 1-373. 1898.—*Includes biographical and bibliographical data on botanists; see also* **Grigoriev, V. V.**

——...Universiteta. Sv. Vladimira, *see* **Ikonnikov, V. S.**

Bogdanov, A. P., editor, 1888-1892: Materials for a history of scientific and practical activity in Russia in zoology and allied branches of science for the last 35 years (1850-88)—I. (Izv. Obshch. Liubit. Estestv. Antrop. & Etnogr. Mosk. Univ. Zool. Otd. 55 (3): i-iv, 222 pp., *pl. 1-12.* 1888; (II) 57 (4): 304 pp., *pl. 13-24.* 1889; (III) 70 (6): 310 pp., *pl. 25-37.* 1891; (IV. pt. i) 71 (7): 126 pp., *pl. 38-41.* 1892). —*A commemorative edition in Russian, including many biographies of botanists*

and explorers with extensive bibliographies and portraits; the pagination and the numbering of volumes are very peculiar, only the first pages of sheets (8 pages) are numbered and each volume has three different numbers.

Bogdanov, V. V., 1915: Piatidesiatilietie Imper. Obshchestva Liubiteleĭ Estestvoznaniia, Antropologii i Etnografii. [The fiftieth anniversary of the Imper. Society of Lovers of Natural Sciences, Anthropology and Ethnography, 1863-1913.] 1-253. — *Includes some data on botanical activity.*

— — 1940: The Society of the Lovers of Natural History, Anthropology and Ethnography at Moscow University. (Uchen. Zapisk. Mosk. Gosud. Univ. 54 (Biol.): 363-369). — *In Russian; a supplement to the preceding, surveys the activity of the Society until its merger with the Society of Naturalists of Moscow in 1931.*

Bolkhovitinov, *see* **Evgeniĭ,** Metropolitan of Kiev.

Bol'shaia Entsiklopediia, *see* **Iuzhakov, S. N.**

Bol'shaia Sovetskaia Entsiklopediia, *see* **Schmidt, O. J.**

Bongard, H. G., 1834: Esquisse historique des travaux sur la botanique entrepris en Russie depuis Pierre-le-Grand jusqu'a nos jours, et de la part que l'Académie a eue aux progrès de cette science. (Rec. Act. Acad. Sci. St.-Pétersb. 1834: 83-108). Reprint 1-26. 1834. — *Includes data on botanical explorations of Russia. For an English excerpt see Hooker's Comp. Bot. Mag. 1, 6: 177-186. 1836.*

Borodin, I. P., 1880: Noveĭshie uspekhi botaniki. [New progress of botany]. (1877-79). i-ii, i-iii, 1-180. *illus.* — *Not seen.*

— — 1898: Istoricheskiĭ ocherk kafedry botaniki v Imperatorskoĭ Voenno-Meditsinskoĭ Akademii. [Historical sketch of the botanical department of the Imper. Military Medical Academy]. 1-40. — *Not seen.*

— —1905: The botanical cabinet of the Imp. Institute of Forestry at the beginning of the second century of its existence. Report for 35 years. (Izv. St.-Peterb. Liesn. Inst. 12: i-vi, 1-160, port.). — *Includes an enumeration of collections and a classified catalogue of the library, pp. 53-160.*

— — 1908: Collectors and collections of Siberian flora. (Trav. Mus. Bot. Acad. Sci. St. Pétersb. 4: i-iii, 1-245.) — *Detailed data on collectors and collections arranged by areas.*

— — 1916-26: Bibliography (for 1916). Jour. Soc. Bot. Russie 1: 119-128, 220-231. 1916; (for 1917) 2: 169-186. 1918; (for 1919) 4: 199-213. 1920; (for 1922) 7: 233-292. 1922; (for 1924) 9: 215-269. 1925; (for 1925) 10: 403-453. 1926.) — *A bibliography of current literature; three last parts in collaboration with* **V. A. Tranzschel.**

Borovskij, M. P., 1878: Istoricheskiĭ obzor 50-lietiia Imper. Obshchestva Sel'skago Khoziaĭstva Iuzhnoĭ Rossii. [A historical review of 50 years' activity of the Imper. Society of Agriculture of South Russia.] — *In Russian; not seen.*

Bourdeille de Montrésor, W., comte, 1892-1900: Les sources de la flore des provinces qui entrent dans la composition de l'arrondissement scolaire de Kieff, contenant les gouvernements de Kieff, de Volhynie, de Podolie, de Tchernigoff et de Poltava. (Bull. Soc. Nat. Moscou n. sér. 6: 322-381. 1892 (1893); 7: 420-496. 1893 (1894); 14: 485-505. 1900.) — *A bibliography in French including 624 works of 345 authors. Part 1-2 reprinted, 1-136. 1893.*

Bretschneider, E. V., 1880: Early European researches into the flora of China. (Journ. N. China Branch Roy. Asiat. Soc. n. ser. 15: 1-194; Reprint. 1-192. 1881). — *Includes historical and biographical data on many Russian botanists and explorers.*

— — 1898: History of European botanical discoveries in China. 1-2: i-xv, 1-1167. — *A comprehensive treatment, issued in 2 volumes paged continuously, with extensive data on explorers and botanists, their collections and writings; articles VI — "Russian botanical explorers in China proper . . . since 1870" and VII — "The Imp. Botanical Garden, St. Petersburg, during the last period" concern Russian botanists and explorers exclusively.*

Brockhaus, F. A. & Efron, I. A., publishers, 1890-1907: Entsiklopedicheskiĭ Slovar'. [Encyclopaedic dictionary.] 41 vols., 2 suppl. vols. — *Edited by Prof.* **I. E. Andreevsky,** *and later by Prof.* **K. K. Arseniev** *and* **F. F. Petrushevsky** *but better known by the name of the publishers; includes botanical data and biographies of botanists and explorers.*

Buchinskij, P., 1896: Brief sketch of the beginning and scientific activity of the Novorossijsk (Odessa) Society of Naturalists for the first 25 years of its existence, 1870-1895. (Mém. Soc. Nat. Nouvelle-Russie (Odessa) 37: i-xlvii). — *A historical sketch in Russian with*

a bibliography of works of the members of the society.

Bulich, N. N., 1904: Iz pervykh liet Kazanskago Universiteta, 1805-1819. [From the early years of Kazan University, 1805-1819]. 1: i-xiv, 1-558. 1904; 2: i-xiv, 1-734. 1904.—*A historical survey with some data on professors and teachers of natural history.*

Bulletin of applied botany, of genetics and plant breeding. Series XIII—Reviews and bibliographies. (Changed to "Abstracts and bibliographies" with no. 2) —*This periodical, started in 1933, is of great help for the study of the botany of Russia.*

Bunge, N. A., editor, 1873-94: Ukazatel' russkoĭ literatury po matematikie. chistym i prikladnym naukam. [Guide to the Russian literature on mathematics, and pure and applied natural history]. 20 vols.—*This subject bibliography, published by the Society of Naturalists of Kiev, includes many works on the botany of Russia; 11 volumes (1873-83) have been examined by me.*

Busch, N. A., 1918: Revue des travaux sur la phytogéographie de la Russie (1915-1917). (Jour. Soc. Bot. Russie 3: 61-179).—*A critical survey in Russian of phytogeographical works with a bibliography of 459 entries.*

——1937: The progress of botany during 20 years of Soviet power. (Priroda 1937(10): 141-150.)—*In Russian.*

Busse, F. F., 1874: Ukazatel' literatury ob Amurskom kraie. [Guide to the literature on Amur region]. i-iii, 1-42; Ed. 2: i-iv, 1-80. 1882.—*First edition is a reprint from Izv. Russk. Geogr. Obshch. vol. 10, no. 7; includes some botanical works.*

Chistovich, Ia. A., 1883: Istoriia pervykh meditsinskikh shkol v Rossii. [History of the earliest medical schools in Russia]. i-ii, 1-662, i-ccclxx.—*Second part of this work includes biographies of many physicians-botanists of 18th century.*

Cohn, F. J., 1902: Istoricheskiĭ ocherk Minussinskago miestnago muzeia za 25 liet (1877-1902). [Historical sketch of the regional museum at Minussinsk for 25 years (1877-1902)]. 1-260, 1-6. —*In Russian; includes data on botanical collections and botanical activity.*

Deĭnega, V. A., *see* **Kursanov, L. I.**

Dechiseaux, P., 1725: Mémoire pour servir à l'instruction de l'histoire naturelle des plantes de Russie et a l'établissement d'un jardin botanique à St. Pétersbourg. 1-33; ed. 2: 1-33. 1728. —*Historical.*

Devrien, A. F., publisher, 1900-12: Polnaia Entsiklopediia russkago sel'skago khoziaĭstva i soprikasaiushchikhsia s nim nauk. [A complete Cyclopaedia of Agriculture and of allied sciences]. 12 vols. Editor—**A. F. Rudzski.**—*Includes data on the history of agriculture in Russia and numerous biographies of agriculturists and agronomists. Another edition published by the Soviet government, 1930-; not seen.*

Dokuchaev, V. V., editor, 1894: Trudy Komissii po izsliedovaniiu S.-Peterburga i ego okrestnosteĭ . . . [Transactions of the Commission for the investigation of St. Petersburg and its vicinity, etc.] Pt. 1: i-iii, 1-488.—*Edited by 8th Congress of the Russian naturalists and physicians; includes a section "Botany", pp. 167-217 with a bibliography of more than 350 entries compiled by* **R. E. Regel** *and "Agriculture", pp. 218-239, also with a bibliography.*

Domrachev, G. V., *see* **Ohl, I. A.**

Dostoevsky, A. A., *see* **Semenov, P. P.**

Efremov, P. A., *see* **Novikov, N.**

Elenkin, A. A., 1914a: Ueber die Tätigkeit des Kryptogamen-Herbariums im Zeitraume von 14 Jahren (von 1899 bis 1913) und . . . (Bull. Jard. Bot. Pierre Grand 14: 1-20).—*In Russian with a German summary; includes data on collections, expeditions and literature.*

——1914*b*: "Die Süsswasseralgen Kamtschatka's" and "Die Meersalgen Kamtschatka's aus der Klassen der Diatomeen und Peridinien." (In *V. L. Komarov et al.,* Expedition à Kamtchatka, etc. 2: 1-448, 579-592. *illus.*).—*In Russian with a German summary; includes a bibliography of 216+45 entries.*

——1923: Iz itogov 25-ti letneĭ deiatel'nosti Instituta Sporovykh Rasteniĭ Glavnogo Botanicheskogo Sada. [From the summary of 25 years' work of the Institute of Cryptogamous Plants of Principal Botanical Garden]. 1-28.—*Contains short historical data and an enumeration of collectors and their collections. See also* **Fedtschenks, B. A.**

——& **Ohl, L. A.,** 1926: Die Fortschritte der floristischen Algologie in U. S. S. R. (Union der Socialistischen Soviet Republiken) während der letzten 25 Jahre. (Bull. Jard. Bot. Princ. URSS 25: 205-217. *1 diagr.*).—*In Russian with a German summary; a statistical review with critical notes on the most important works.*

——1929: Bibliographie des ouvrages algologiques en URSS parus depuis 1900 jusqu'à 1925 inclusivement. (Act. Hort. Petrop. 42: i-iv, 1-139).—*In Russian*

with a French résumé, contains 1327 entries. A continuation of **Gaïdukov, N. M.**

—— 1935: Bibliographie der in der USSR von 1926 bis 1930 inkl. erschienenen algologischen Arbeiten. (Act. Inst. Bot. Acad. Sci. URSS, Ser. II, fasc. 2: 171-255). — *In Russian, with a brief German résumé, a continuation of the preceding entry.*

Elfving, F., 1921: Societas pro Fauna et Flora Fennica 1821-1921. (Act. Soc. Fauna & Fl. Fenn. 50: [1-6], 1-279. *3 pl.*). — *A comprehensive historical work in Swedish, including a biographical list of about 150 deceased Finnish naturalists and a complete list of the members of the Society.*

Encyklopedya Powszechna Ultima Uhle. [A general encyclopaedia]. 8 volumes. 1930-37. — *An unfinished Polish encyclopaedia (letters A-Q), including some data on the history of botany in Poland and biographies of Polish naturalists. See also* (S.) Orgelbranda Encyklopedja Powszechna.

Entsiklopedicheskiĭ Slovar', *see* **Granat, A. & I.** and **Brockhaus, F. A. & Efron, I. A.**

Evgeniĭ (Bolkhovitinov), Metropolitan of Kiev, 1845: Slovar' russkikh svietskikh pisateleĭ, sootechestvennikov i chuzhestrantsev, pisavshikh v Rossii . . . [Dictionary of Russian secular writers and foreign authors who wrote in Russia . . .]. 1: i-vi, 1-328; 2: 1-290, i-xvi. — *Includes biographical and bibliographical data on Russian botanists.*

Ezhegodnik gosudarstvennoĭ tsentral'noĭ knizhnoĭ palaty RFSR, 1927-1929: Fasc. 1 (for 1925) — Fasc. 3 (for 1927). — *For the earlier issues see 'Bibliograficheskiĭ ezhegodnik.'*

Falck, J. P., *see* **Akademiia Nauk**, 1818-25.

Famintzyn, A., 1891-95: Obzor botanicheskoĭ dieiatel'nosti v Rossii za 1890 g. [Survey of the botanical activity in Russia in 1890]. i-xxi, 1-157. 1891; (ditto for 1891). i-xxvi, 1-264. 1893; (ditto for 1892) i-viii, 1-187. 1894; (ditto for 1893) i-xxi, 1-166. 1894-95. — *A critical survey of the botanical works published in Russia; for German translation see the next entry.*

—— 1892-94: Uebersicht der Leistungen auf dem Gebiete der Botanik in Russland während des Jahres 1890. i-xxiv, 1-173. 1892; (ditto for 1891) i-xxix, 1-294. 1893; (ditto for 1892). i-xxx, 1-213. 1894. — *A German translation of the preceding entry.*

Fedtschenko, B. A., 1904-05: Flora of western Tian Shan. Botanical results of the expedition of 1897 and 1902 and the summary of former expeditions. I. (Act. Hort. Petrop. 23: 249-532. 1904; (II) 24: 155-260. 1905). — *Contains enumeration of collectors and collections and a bibliography of 150 entries; reprint — Pt. 1: 1-284; pt. 2, 1-106. 1905.*

—— 1925: New botanical literature on Siberia from 1918 to 1925. (North Asia 5-6: 207-213). — *A Russian bibliography of current literature.*

—— 1937: Les progrès de la floristique soviétique (1917-1937). Sovietsk. Bot. 1937(5): 5-31. — *Includes data in Russian on the botanical explorations and literature.*

—— **& Elenkin, A. A.**, 1906-08: Aperçu bibliographique de tous les travaux concernant la flore russe parus en 1904. (Bull. Jard. Bot. St.-Pétersb. 5. Suppl.: 1-85. 1905); (ditto for 1905) 6. Suppl.: 1-106. 1906: (ditto for 1906) 1-69. 1908). — *A bibliography of current literature.*

Filipchenko, J. A., 1934: Literature on genetics of wheat. (In symposium Genetics of soft wheat, pp. 248-260). — *This bibliography in Russian has not been seen.*

Fischer, F. E. L., 1844: Notiz über den Kaiserlichen botanischen Garten zu St. Petersburg. (Verhandl. Ver. Beförder. Gartenb. Königl. Preuss. Staaten 17: 275-284. *2 pl.*). — First known *description of St. Petersb. Bot. Garden, including some historical data.*

Fischer von Waldheim, A. A. & others, 1899: Istoricheskiĭ ocherk Imperatorskago S.-Peterburgskago Botanicheskago Sada za posliednee 25-lietie ego s 1873 po 1898 g. [Historical sketch of the Imp. St.-Petersburg Botanical Garden for the last 25 years (1873-1898)]. i-vi, 1-308, i-vi, map. — *This work in Russian includes a description of the herbarium and its collections and a survey of literary activity of the Garden, pp. 206-296, with biographies of various botanists and bibliographies of their works.*

—— 1913-15: Imperatorskiĭ S.-Peterburgskiĭ Botanicheskiĭ Sad za 200 liet ego sushchestvovaniia (1713-1913). [The Imperial St.-Petersburg Botanical Garden during 200 years of its existence (1713-1913)]. 1: i-iii, 1-408, [1-4], *5 port. f. 1-54.* 1913; 2: 1-321, [1-4], *f. 1-66.* 1913; 3: 1-582. *9 pl. (port.) 52 f. (port.).* 1913-15. — *A luxurious commemorative edition in Russian compiled by the members of the staff. Vol. 1 contains a "Historical sketch" by* **V. I. Lipsky**; *Vol. 2 — description of the herbarium, library, etc., by various authors; Vol. 3 — "Biographies and lit-*

erary activity of the botanists and the persons connected with the Imp. Botanical Garden," by **V. I. Lipsky** — *a very important biographical and bibliographical work.*

Fischer von Waldheim, G., 1806-07: Museum Demidoff . . ou catalogue . . . des curiosités de la nature et de l'art, données a l'Université Impériale de Moscou par . . . P. de Demidoff. 1: i-lxxiii, 1-275. 1806; 2: i-xviii, 1-300. *6 pl.* 1806; 3:i-ix, 1-330. *6 pl.* 1807; ed. 2 in 1817. — *Includes a historical sketch and description of botanical collections.*

Flerov, A. F., 1911: Vegetationsbilder aus dem Transbajkalgebiete. Urwald. (In **Fedtschenko, B. A. & Flerov, A. F.** Russlands Vegetationsbilder. Ser. I. 4: 1-43. *pl. 19-25. f. 1-5.*). — *In Russian and German; includes a bibliography of 170 entries.*

Gaïdukov, N. M., 1901: Litteratur — Quellen zur Algenflora Russlands. (Script. Bot. Hort. Univ. Petrop. 17: 1-126). — *A bibliography in Russian with a German résumé; includes 452 entries and a geographical index. For a continuation see* **Elenkin, A. A. & Ohl, L. A.**

Geidrich, E. K., *see* **Akademiia Nauk** 1930.

Gennadi, G., 1858: Literatura russkoĭ bibliografii. Opis' bibliograficheskikh knig i stateĭ izdannykh v Rossii. [Literature of Russian bibliography. Enumeration of bibliographical books and articles published in Russia]. [1-2], 1-196. — *A general work including some data on bibliography of botany.*

—— 1858-77: Brief data on Russian writers and scientists deceased in 1857. Bibliogr. Zapisk. 1858(12): 378-382; *for 1858* — *ibid.* 1859(20); 646-662; *for 1859* — *ibid.* 1861(4): — and Knizhn. Viestn. 1860(11-12): 107; *for 1860-62* — Russk. Arch. 1864(5-6): 641-674; *for 1863* — *ibid.* 1865(1): 115-128; *for 1864* — *ibid.* 1866(4): 561-580; *for 1865* — *ibid.* 1867(5-6): 958-981; *for 1866* — *ibid.* 1868(12): 2001-2026; *for 1867* — *ibid.* 1869(11-12): 1985-2017; *for 1868* — *ibid.* 1870(11): 2013-2043; *for 1869* — *ibid.* 1871: 483-510; *for 1870* — *ibid.* 1872: 1985-2023; *for 1871* — *ibid.* 1872: 1479-1515; *for 1872* — *ibid.* 1874: 1095-1124; *for 1873* — *ibid.* 1874: 461-468; *for 1874* — *ibid.* 1877(2): 79-94. — *Later these data were included in the following entry.*

—— 1876-1906: Spravochnyĭ slovar' o russkikh pisateliakh i uchenykh umershikh v 18 i 19 stolietiiakh i spisok russkikh knig s 1725 po 1825. [Reference book of Russian writers and scientists, deceased in 18th and 19th centuries, and bibliography of Russian books from 1725 to 1825]. 1: i-vii, 1-351. 1876; 2: 1-434. 1880; 3: 1-291. 1906. — *An unfinished work (letters A-R); third volume published posthumously by* **A. Titov**; *includes biographies of Russian botanists and bibliographies of their works.*

Glinka, G. V. & others, 1914: Aziatskaia Rossiia. [Asiatic Russia]. 1: i-viii, 1-576, i-ii, *illus.*; 2: 1-638, i-ii, *illus.*; 3: i-clv; atlas 1-4, 1-24, *maps 1-70.* — *A full description of Asiatic Russia by various authors, edited by "Pereselencheskoe Upravlenie" [Migration Dept.], including botanical data, history of exploration (2: 617-638) and a bibliography, including botanical works (3: lxxi-cxxxv); atlas has not been seen.*

Gluzdovskiĭ, V. E., 1917: Primorsko-Amurskaia okraina ĭ sievernaia Manchzhuriia. [Primorsk-Amur region and northern Manchuria]. Ed. 2, i-ii, 1-184, *103 f. 4 maps. 10 diagr.*]. — *Includes data on the exploration of the region; the first edition has not been seen.*

—— 1927: Dal'nevostochnyĭ kraĭ. [Far Eastern region]. Ed. 2, 1-268. *2 maps. text-maps 1-15. 100 f.* — *Includes a bibliography; the first edition has not been seen.*

Gnucheva, V. F., 1940: Materialy dlia istorii ekspeditsiĭ Akademii Nauk v 18 i 19 vekakh. [Materials for the history of the expeditions of the Academy of Sciences in 18th and 19th centuries]. — *Includes some data on botanical exploration of Russia; not seen.*

Golovachev, A. F., 1860: Bibliograficheskiĭ ukazatel' po estestvennym naukam v Rossii s 1856-1860 g. [Bibliographical guide to natural history of Russia 1856-1860]. 1-116. —*Includes section "3 — Botany".*

Gorlenko, M. V., 1946: Twenty five years in the study of cereal diseases in USSR (1917-1942) (Jour. Bot. URSS 31: 3-17). — *In Russian with a brief English summary; a critical review of the achievements in this field of phytopathology with a bibliography.*

Gorozhankin, I. N., 1884: Mémoire sur l'état des herbiers de l'Université Impériale de Moscou et de la Société des Naturalistes de Moscou. (Bull. Soc. Nat. Moscou 59: 290-303). — *In French; a brief historical survey and enumeration of collections.*

Granat, A. & I., publishers, 1911-29: Entsiklopedicheskiĭ Slovar'. [Encyclopaedic dictionary]. Ed. 7. 52 vols. — *Edited*

by the Russian bibliographical institute of A. & I. GRANAT; *includes many botanical articles and biographies of botanists.*

Gribanovskiĭ, N. N., 1932: Bibliography of Iakutsk region. Part 1. Natural resources and population of Iakutsk region. (Trudy Sovet. Izuch. Proizv. Sil Iakutii 9: [1-3], i-viii, 1-127). — *Includes some botanical works.*

Grigoriev, A., 1916: Imperial Russian Geographical Society (1845-1915). (Priroda 1916: 388-394). — *A short review of the activity of the Society, including some data on botanical explorations.*

Grigoriev, V. V., 1870: Imperatorskiĭ S.-Peterburgskiĭ Universitet v techenie pervykh piatidesiati liet ego sushchestvovaniia. [Imp. St.-Petersburg University during the earliest 50 years of its existence]. [1-3], 1-432, 1-96, i-cxxii. — *A historical sketch including data on the history of botanical department.*

Hasselblatt, A. & Otto G., 1889: Album Academicum der Kaiserlichen Universität Dorpat. (1802-1889). i-viii, 1-1007. — *A biographical index, including some botanists connected with the University; two previous editions (1852 and 1853) by* **Rummel, C.**, *have not been seen.*

Heintz, G. V. et al., 1935: Bibliography of world literature on the genetics and breeding of wheat. (Bull. Appl. Bot. & Pl. Breed. Ser. V-A(1): [1-8], 1-135). — *A comprehensive bibliography of 2313 entries, including many Russian works.*

Herder, F. von, 1881: Fontes florae rossicae; continuatio 1846-1879. (Bot. Centralbl. 5: 155-158, 185-188, 220-222, 281-284, 316-318, 346-348, 385-388, 406-408; 6: 31-34, 63-67, 137-138, 176-178, 245-247, 277-296, 324-329.). Reprinted 1-58. 1881. — *For earlier references see* **Ledebour, K. F. von.**

— — 1888: Biographische Notizen über einige in den Plantae Raddeanae genannte Sammler und Autoren. (Bot. Jahrb. Engl. 9: 429-456). — *Includes biographical notes and data on collections of more than 100 collectors and explorers.*

— — 1893: Die in St. Petersburg befindlichen Herbarien und botanischen Museen. (Bot. Centralbl. 55: 257-269, 289-298). — *Short historical data and an enumeration of the collections in the herbaria of the Medical Academy, the Botanical Museum of the Academy of Sciences, the Botanical Garden, the University and the Institute of Forestry.*

— — 1894: Uebersicht über die botanische beschreibende Litteratur und die botanische Sammlungen des Kaiserlichen botanischen Gartens in St. Petersburg, nach den Gouvernements und Gebieten des europäischen und asiatischen Russlands zusammengestellt. (Bot. Centralbl. 58: 385-392). — *Enumeration of literature sources and botanical collections arranged by regions.*

Hoffman, G. F., 1807: Oratio in universitate Mosquensi habita de hortis botanico-medicis. 1-32, 1-2. *map.* (Russian translation 1-32). — *Includes data on history of Russian medical gardens.*

— — 1808: Hortus Mosquensis. 1-42, [1-24]. *pl.* — *Includes some historical data and description of the garden.*

— — 1823: De fatis et progressibus Rei Herbariae, imprimis in Imperio Rutheno. Oratio. 1-32; (Russian summary of this discourse in Viestnik Evropy 1823 (no. 13-14): 74-80.) — *Includes data on Russian botanical explorations and botanical gardens.*

— — 1824-25: Herbarium vivum, sive collectio plantarum siccarum, Caesareae Universitatis Mosquensis, etc. (1: 1-179. 1824; 2: 1-467. 1825. — *Includes data on the development of the herbarium and its description.*

Hryniewiecki, B., 1927: Historja botaniki w Polsce — L'histoire de la botanique en Pologne. (Poradnik dla samouków 7(Bot. 2): 599-743). — *Not seen.*

— — 1931-33: Précis de l'histoire de la botanique en Pologne. Publié par la Société Botanique de Pologne à l'occasion du 3-ème Congrès des Botanistes Slaves tenu à Varsovie du 24 au 26 juin 1931. Augmenté de 57 portraits en 1933. 1-45. *57 ports.* — *Not seen.*

— — 1933*a*: Précis de l'histoire de la botanique en Pologne. L'histoire sommaire des Sciences en Pologne publiée à l'occasion du VII-e Congrès International des Sciences Historiques. 1-23. — *Not seen.*

— — 1933*b*: Tentamen florae Lithuaniae. Zàrys flory Litwy. (Arch. Biol. Soc. Sci. & Lett. Varsovie 4: i-xvi, 1-368. *port. illus.*). — *In Polish with a French résumé, pp. 316-332; includes a history of botanical investigation of Lithunia with a bibliography of 242 entries and some biographical data.*

Hultén, E., 1927-30: Flora of Kamchatka and the adjacent islands. Pts. 1-4. (Svensk. Vet. Akad. Handl. III.5(1): 1-346. *pl. 1-6. f. 1-18. text-maps 1-291.* 1927; (II) *ibid.* III.5(2): 1-218. *pl. 1-3. f. 1-14. text-maps 292-459.* 1928; (III) *ibid.* III.8(1): 1-213. *pl. 1-3. f. 1-18. text-maps 460-598.*

1929; (IV) *ibid.* III.8(2): 1-358. *pl. 1-6. f. 1-18. text maps 599-791.* 1930.—*Pt. I has a "Historical Summary" with a table of collectors and their collections and pt. IV—an extensive bibliography, pp. 258-270.*

——1940: History of botanical exploration in Alaska and Yukon territories from the time of their discovery to 1940. (Bot. Notis. 1940: 289-346. *fold. map*).—*Includes data on many Russian botanists and explorers.*

Iasnitskiĭ, N. V., 1926: Brief survey of the botanical explorations of the Eastern Siberian Branch of the Russian Geographical Society for 75 years of its existence. (Izv. Vost. Sibirsk. Otd. Russk. Geogr. Obshch. 50 (1): 95-108).—*Contains data on many expeditions in eastern Siberia.*

Iazykov, D., 1884?-1916?: Obzor zhizni i trudov russkikh pisateleĭ i pisatel'nits. [Survey of lives and works of Russian men and women-writers]. Fasc. 1-13.?—*Includes data on some botanists; each fascicle paged separately and the titles of them vary slightly; only fasc. 13: 1-314. 1916 has been seen.*

Ikonnikov, V. S., 1884: Biograficheskiĭ slovar' professorov i prepodavateleĭ Imp. Universiteta Sv. Vladimira. [Biographical dictionary of professors and instructors of the University of St. Vladimir (Kiev)]. (1834-84). i-xxxvi, 1-816, i-ii.—*Includes biographies of botanists and very complete bibliographiea of their works; see also* **Rogovich, A. S.**, *1864, and* **Vladimirskiĭ-Budanov, M. F.**

——1891-1908: Opyt russkoĭ istoriografii. [An essay on Russian historiography]. 1(1): i-viii, 1-882, i-ccxxii, (Dop.-Suppl.), i-viii. 1891; 1(2): 883-1539, ccxxiv-ccclxxi (Dop.), 1-149 (ind.), i-x. 1892; 2(1): i-v, i-x, 1-1056, i-xxxii (Dop.), i-v. 1908: 2(2): i-iii, 1057-1955, i-ix, 1-113 (ind.), xxxiii-xlix (Dop.), i-xi 1908.—*Volume 1 includes important bibliographical data on the libraries, archives, and museums of the Academy of Sciences, universities and scientific societies.*

Iljin, M. M., 1942: Résultats de l'étude de matières premières végétales de l'URSS faites pendant la période Soviétique. (Sovietsk. Bot. 1942(6):24-40).—*A summary in Russian on the results of the study of raw materials of USSR from 1917 to 1942.*

Iljinsky, A. P., 1937: 20 ans de cartographie géobotanique soviétique. (Sovietsk. Bot. 1937(5): 94-103).—*A critical survey of the progress of botanical cartography.*

——1942: Ce qui a été fait par les botanistes soviétiques dans le domaine des problèmes générales de phytogéographie. (Sovietsk. Bot. 1942(6): 12-23).—*An historical sketch in Russian covering last 25 years.*

Isachenko, B. L., 1945: An outline of the history of microbiology in Russia. (Bull. Acad. Sci. URSS. Sér. Biol. 1945(2): 229-246.—*A review in Russian with an English summary of the progress of microbiology beginning with the end of the 17th century.*

Iso Tietosanakirja [Great Encyclopaedia], 1931-39. 15 vols.—*A Finnish encyclopaedia, including biographies of many Finnish naturalists and explorers.*

Ivanovskiĭ, editor, 1898: Istoriia Voenno-Meditsinskoĭ Akademii za sto liet, 1798-1898. [History of the Military Medical Academy for one hundred years, 1798-1898]. 1-828. (Suppl.) 1-337.—*In Russian; a comprehensive historical work, including data on the development of the Department of Botany of this Academy; not seen.*

Ivashkevich, B. A., 1927: What is done and what is necessary to do for the study of the forests of the Far East. (Proizv. Sily Dal'n. Vost. 3: 21-52).—*Includes data on the exploration of the Far East.*

Iuzhakov, S. N., 1902-1909: Bol'shaia Entsiklopediia. [Great Cyclopaedia]. 22 vols.—*Includes biographies of important Russian botanists and articles on the botany of Russia.*

Jaczewski, A. A., 1931: Cotton diseases. (Bull. Appl. Bot. & Pl. Breed. 24(5): 3-294. *f. 1-26*).—*In Russian; includes historical data and a bibliography of 218 entries. See also* **Mishchenko, P. I.**

Kabanov, N. E., 1935*a*: Bibliography of the materials on the vegetation and soil cover of the Soviet Far East for the last decade (1923-1933). (Trudy Dal'nevost. Fil. Akad. Nauk SSSR Ser. Bot. 1: 433-568).—*A comprehensive bibliography in Russian, including 1363 entries.*

——1935*b*: Dixième anniversaire (1925-1935) de l'étude du sol et de la végétation du Sachalien de Sovets. (Bull. Far East. Branch Acad. Sci. USSR 15: 37-51. *1 map*).—*In Russian with a French résumé; includes data on botanical expeditions and a bibliography of 47 entries.*

Karsky, E. F., *see* **Nauka.**

Khodnev, A. I., 1865: Istoriia Imperat. Vol'nago Ekonomicheskago Obshchestva s 1765 do 1865 g. [History of the Imper. Free Economic Society from

1765 to 1865]. i-iii, i-ix, 1-667.—*Includes data on botanical activity of the Society; not seen.*

Khotinskiĭ, M., 1853: Survey of the history of Dorpat University since its establishment in 1802. (Zhurn. Minist. Narodn. Prosveshch. 1853: no. 2 & 8). —*A partial translation and review of the paper by* **Beise, T.**, *1852.*

Kizell, A. R., 1940: Biochemistry of plants at Moscow University. (Uchen. Zapisk. Mosk. Gosud. Univ. 54 (Biol.).: 315-321).—*Historical.*

Kizevetter, A., 1930: Moscow University. (A historical sketch). (Moskovskiĭ Universitet, 1755-1930. Iubileĭnyĭ sbornik, pp. 9-140).—*In Russian: includes some data on the progress of botany.*

Klinge, J. C., *see* **Berg, E.**

Kniazev, G. A., *see* **Liubimenko, I. I.**

Knizhnaia Lietopis' Glavnago Upravleniia po dielam pechati. [Chronicle of non-periodicals registered with the Central Office of the Press.] 1 (1907) + —*A classified list of titles, including works on natural history (sect. 28) and agriculture (sect. 16).*

Komarov, V. L., 1901-07: Flora Manshuriae. I. (Act. Hort. Petrop. 20: 1-559. 1901; (II) *ibid.* 22: i-iii, 1-787. *pl. 1-17.* 1903-04; (III) 25: 1-853. *pl. 1-16.* 1907).—*A critical flora, the text in Russian: vol. 1 includes a history of botanical investigations by Russians, pp. 19-34, enumeration of collections and a bibliography, pp. 34-64.*

—— 1908: Prolegomena ad floras Chinae nec non Mongoliae. Fasc. 1. (Act. Hort. Petrop. 29: 1-176. *pl. 1-4. 2 maps*).—*Includes historical data on botanical investigations, critical review of the literature and enumeration of collections.*

—— 1920-28: Les itinéraires botaniques des principales expéditions russes en Asie Centrale. Livre premier. Itinéraires de **N. M. Przhevalski.** (Act. Hort. Petrop. 34: 1-192. 1920) ; *ditto.*—Livre second. Itinéraires de **G. N. Potanine.** 1876-1899. (*ibid.* 34: 197-404. *fold. map.* 1928).—*In Russian with additional introduction in French; includes an account of journeys in western China, Tibet and Mongolia.*

—— 1922: Short outline of the vegetation of Siberia. (Mater. Izuch. Estestv. Proizv. Sil Ross. 1-97).—*Includes a list of literature, pp. 90-97.*

—— 1923: Plantae Austro-Ussurienses. (Lacus Chanka-Wladiwostok-sinus S'ti. Vladimiri). (Act. Hort. Petrop. 39: 1-128).—*In Russian with a German summary; includes enumeration of collectors and collections.*

—— 1926*a*: Introduction à l'étude de la flore de l'Iakoutie. (Trav. Comm. Étud Iakoute 1: i-x, 1-183, *pl. 1-8, 2 maps.*). —*In Russian with an English summary; includes a historical sketch and a bibliography of 372 entries.*

—— 1926*b*: Botany. (Pacific Russian scientific investigations. (Published by the Academy of Sciences USSR), pp. 121-136. *port.*).—*A historical account of Russian botanical expeditions with a bibliography; published also in Russian under the same title in "Tikhiĭ Okean", pp. 111-124. 1926.*

—— 1927-30: Flora poluostrova Kamchatki. [Flora peninsulae Kamtschatka]. 1: 1-2, 1-339. *pl. 1-13. fold. map.* 1927; 2: i-iii, 1-369. *pl. 1-32.* 1929; 3: 1-210. *pl. 1-9.* 1930.—*In Russian with an English summary; includes a historical account and a critical review of literature (1: 1-14).*

—— 1928: Bibliography of the flora and of vegetation of the Far East. (Mem. S. Ussuri Branch Russ. Geogr. Soc. 1928 (2): 1-279).—*A comprehensive work in Russian containing over 1150 entries, with indexes to subjects and authors.*

Konstantinov (Coff), P. F., 1945-46: "Petrovka" i "Petrovtsy". (Russkaia Zhizn'—Russian Life) 25: nos. 240, 242. *port.* 1945; *ibid.* 26: nos. 1, 2. *port.* 1946.—*A brief historical sketch in Russian of Petrovsk-Razumovsk. Agric. Academy (now Timiriazev Agric. Acad.) for 80 years of its existence (1865-1945) with biographical data on some of its professors and graduates.*

Korsakov, K. D., 1928: Bibliography of tea, from 1589 to 1926 inclusive. (Bull. Appl. Bot. & Pl. Breed. 18(3) : i-ii, 273-473; Reprint, 1-201. (1927) 1928).—*A comprehensive bibliography of Russian, pp. 275-382, and foreign works, pp. 383-431, on the tea industry, including some papers on the systematics, cultivation and acclimatisation of tea plant.*

Korzhinsky, S. I., 1898: Tentamen florae Rossiae orientalis, id est provinciarum Kazan, Wiatka, Perm, Ufa, Orenburg, Samara partis borealis atque Simbirsk. Mém. Acad. Sci. St. Pétersb. VIII. 7 (1): i-xix, 1-566. *2 maps*).—*Includes "Litteratura florae Rossiae orientalis," pp. iv-xiv, and "Fontes florae Rossiae orientalis," pp. xiv-xviii.*

Kosovanov, V. P., 1923-1930: Bibliographie des Jenisseier Gebietes; sistematischer Anzeiger der Bücher und

Abhandlungen, die in russischer und fremden Sprachen in der Zeit von 1612 bis 1923 einschlisslich veröffentlicht sind. Bd. 1 — *not seen;* 2 (Philologie, exakte Wissenschaften und angewandte Kunde): I-XII, 1-296, I-XV. 1923; 3 (Kunst, Litteratur, beschreibende Geographie, Kartographie und Biographien): I-XVI, 1-348, I-XIV. 1930. — *A comprehensive bibliographical work in Russian with somewhat misleading title because it covers not only Enisseisk but also Irkutsk, Tomsk, Altai, Jakutsk provinces and north-western Mongolia; includes sections — "botany" (2: 107-122), "pomology" (2: 257-259), "forestry" (2: 259-264), etc.; Vol. 3 includes a very valuable section "biography", pp. 241-348 with data on many botanists and explorers.*

Kotov, M. I., 1937: What has been done by Kharkov botanists for the study of the flora and vegetation of the Ukraine and the Union from the time of revolution (1917-35). (Journ. Bot. URSS 22: 116-124). — *A historical sketch with a critical review of the literature and a bibliography.*

Kozhin, A. E., 1931: Citrus plants and their cultivation in the USSR. (Bull. Appl. Bot. & Pl. Breed. 26(1): 241-540). — *Includes a bibliography of Russian works on citrus plants, pp. 475-497, 540.*

Krasheninnikov, S., *see* **Akademiia Nauk**, 1818-25.

Krasnov, N. A., 1888: Essay on the history of the development of the flora of the southern portion of the eastern Tian-shan. (Zapisk. Russk. Geogr. Obshch. 19: i-v, 1-413. *7 pl. map.*). — *In Russian; includes an article on the history of botanical exploration of the region, pp. 91-105, with a small bibliography.*

Krebel, R., 1847: Russlands naturhistorische und medicinische Literatur. Schriften und Abhandlungen in nichtrussischer Sprache. i-vi, 1-220. — *A subject bibliography, including works on the botany of Russia.*

Krylov, P. N., 1901-14: Flora Altaia i Tomskoĭ gubernii. Rukovodstvo k opredieleniiu rasteniĭ Zapadnoĭ Sibiri. [Flora of Altai and Tomsk Province. A manual for the determination of the plants of western Siberia]. 7 vols. — *The preface includes a historical sketch of exploration of the region and a bibliography, pp. i-xxii.*

Kryshtofovich, A. N., 1937: Twenty years of Soviet palaeobotany. (Priroda 1937 (10): 150-163. *illus. map.*). — *A critical review in Russian of the progress in this field of science.*

Kunik, A. A., 1865: Sbornik materialov dlia istorii Imperatorskoĭ Akademii Nauk v XVIII viekie. [Collection of materials for the history of the Imper. Academy of Sciences in 18th century]. Pt. 1: i-viii, 1-224; pt. 2: i-vi, 225-530. — *Includes some data on Botanical section of the Academy; not seen.*

Kursanov, L. I., 1940: Study of cryptogams at the Moscow University. Uchen. Zapisk. Mosk. Gosud. Univ. 54(Biol.): 294-304. *port.*). — *Historical.*

— — & **Deĭnega, V. A.**, 1940: Society of Naturalists of Moscow. (Uchen. Zapisk. Mosk. Gosud. Univ. 54(Biol.): 353-362). — *A brief historical sketch.*

Kushelev-Bezborodko, A., Count, 1859: Litseĭ kniazia Bezborodko. [Lyceum of Prince Bezborodko]. 1-208. *port.* — *Contains a historical sketch and biographies of professors, including some botanists.*

Kuznetsov, B. G., 1940: Ocherki istorii russkoĭ nauki. [Sketches of the history of Russian science]. 1-171. *23 ports.* — *Contains a chapter "On the history of Russian biology" with data on the progress of botanical science in Russia and biographical notes.*

Kuznetsov, N. I., 1890-98: Review of the works on phytogeography of Russia for the year 1889. — I. (Ezhegodn. Russk. Geogr. Obschch. 1890: 151-171; for 1890 — II, ibid. 1891: 177-229; for 1891 — III, ibid. 1893: 207-250; for 1892-93 — IV-V, ibid. 1894: 71-151; for 1894 — VI, ibid. 1896: 133-189; for 1895-96 — VII, ibid. 1898: 35-118.). — *A critical review of the current literature; the last part in collaboration with* **N. A. Busch.**

— —, editor, 1900-14: Review, critical essays, and bibliography of the works on the botany of Russia in Acta Horti Botanici Universitatis Imperialis Jurjevensis, vols. 1-15. — *This review section at the end of each fascicle is one of the best sources of information on the botany of Russia for that period. See also* **Voronin, M. S.**

Lamansky, V. I., *see* **Semenov, V. P.** and **P. P.**

Lavrenko, E. M., 1940: Steppes of USSR. (Vegetatio URSS 2: 1-265. *f. 1-45. t. 1-5*). — *A comprehensive work in Russian, including a historical sketch of steppe investigation and a bibliography of about 450 entries.*

— — 1943: Development of the essential ideas of Soviet geobotany (phytocoenology) for 25 years (1917-42).

(Pochvovedenie 1943(3): —). — *In Russian; not seen.*

Lavrov, N. N., 1937-38: Florae fungorum et myxomycetum Sibiriae et regionum confinium Europae, Asiae, Americaeque fontes. Fas. 1. (Trav. Inst. Sci. Biol. (Tomsk) 3 (1): 12-59. 1937; (Fasc. 2): 1-32. 1938). — *In Russian with Latin preface and résumé; includes a bibliography of 593 entries and a list of 312 collectors and explorers with detailed data on travels and collections.*

Ledebour, K. F., 1841-46: Florae rossicae fontes. In his Flora rossica, 1: vii-xvi. 1841; 2: iii-vi. 1846. — *A list of literature on the flora of Russia; for continuation see* **Herder, F. von,** *1881.*

Lehmann, E., 1895-97: Flora von Polnisch-Livland, mit besonderer Berücksichtigung der Florengebiete Nordwestrusslands, des Ostbalticums, der Gouvernements Pskow und St. Petersburg, sowie der Verbreitung der Pflanzen durch Eisenbahnen. i-xiii, 1-556. *map.* (Reprinted from Archiv für Naturkunde Liv-, Esth- und Kurlands Bd. 11, Lfg. 1 & 2). — *Includes a bibliography of 236 entries, pp. 3-17, 445-448 and some data on the exploration of the region.*

Lepekhin, I. I., *see* **Akademiia Nauk,** 1818-25.

Lepik, E., 1929: Bibliographische Beiträge zur ostbaltischen Pilzflora. (Sitzungsber. d. Naturf. Ges. Tartu 36: 27-88). — *Not seen.*

Lepin, T. K., 1932: Genetics in the USSR for fifteen years (1917-1932). (Priroda 1932: 1115-1139). — *A historical sketch with a critical review of various works on plant genetic.*

Levitsky, G. I., 1934: Soviet cytology in plant breeding for the years 1928-1933. (Bull. Appl. Bot. & Pl. Breed. Ser. A. 10: 25-39). — *A critical review in Russian of the progress of cytology with a bibliography, pp. 36-39.*

Levitsky, G. V., 1902-03: Biograficheskiĭ slovar' professorov i prepodavateleĭ Imp. Iur'evskago, byvshago Derptskago, Universiteta za sto liet ego sushchestvovaniia. [Biographical dictionary of professors and instructors of the University of Juriev (formerly Dorpat) for a century of its existence]. (1802-1902). 1: i-vi, 1-666. 1902; 2: i-vi, i-xvi, 1-656. 1903. — *Includes biographical sketches of botanists, compiled mostly by* **N. A. Busch;** *see also* **Hasselblatt, A. & Otto, G.**

Lindemann, E. von, 1863-85: Kurze Nachrichten ueber den Bestand meines Herbariums. (Bull. Soc. Nat. Moscou 36 (1): 233-253. 1863: 45 (2): 56-101. 1872 (1873); 60 (2): 265-312. 1884; 61 (1): 37-92. 1885). — *An enumeration of all collectors who contributed to this herbarium with brief biographies and data on their travels.*

Lipschitz, S. J., 1940: Société des naturalistes de Moscou. (1805-1940). Aperçu historique. 1-131, [1-4], *14 port.* — *A historical sketch in Russian, including data on several expeditions sponsored by this oldest natural history society of Russia and biographical notes on many botanists and explorers.*

Lipsky, V. I., 1898: Gerbariĭ Imperatorskago S.-Peterburgskago Bontanicheskago Sada k kontsu ego 75-lietniago sushchestvovaniia (1823-1898). [Herbarium of the St.-Petersburg Botanical Garden to the end of its 75 years' existence.] 1-128, i-vi. 1898; ed. 2, 1-238. 1908. — *This work contains a general description of herbarium, an alphabetical enumeration of all collections (name, of collector, region, date, etc.) and distribution of collections in geographical order.*

— — 1899-1902: Flora Caucasi. (Trudy Tiflissk. Bot. Sad. 4: i-xv, 1-585. 1899; Suppl. I. 5: 1-100. 1902). — *Pages 1-198 of this work in Russian contain a bibliography of 396 entries and data on collections and collectors.*

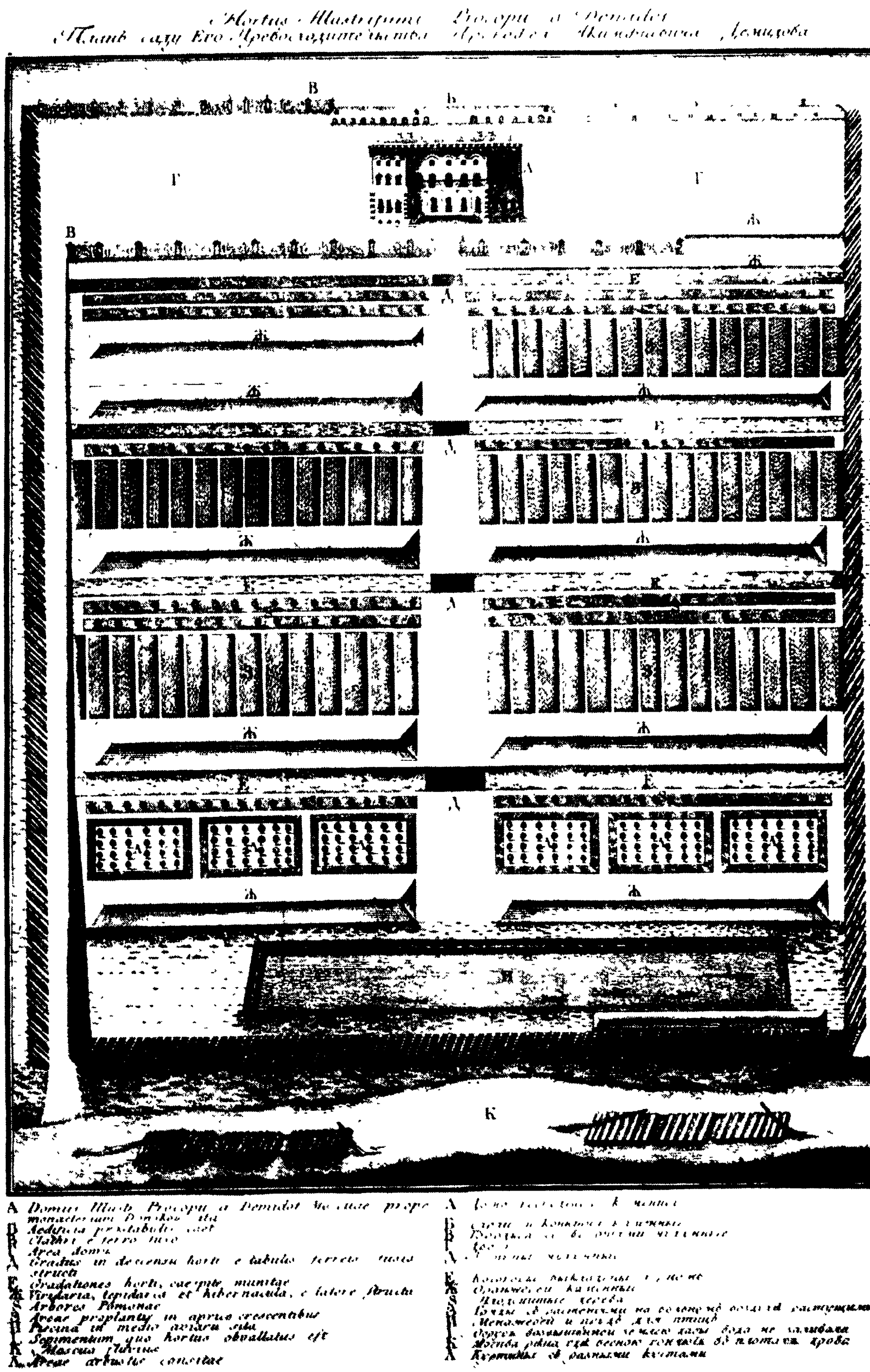
— — 1902-05: Flora Asiae Mediae seu Turkestaniae Rossicae inclusis chanatis Buchara et Chiwa. (Trudy Tiflissk. Bot. Sad. 7: 1-841). — *In Russian; part 1, pp. 1-246 (1902), contains 'Literatura florae Asiae Mediae'; part 2, pp. 247-338 (1903), contains 'Historia florae exploratae'; part 3, pp. 339-841 (1905), contains 'Collectiones botanicae Asiae Mediae.'*

— — *see also* **Fischer von Waldheim, A. A.,** 1913-15.

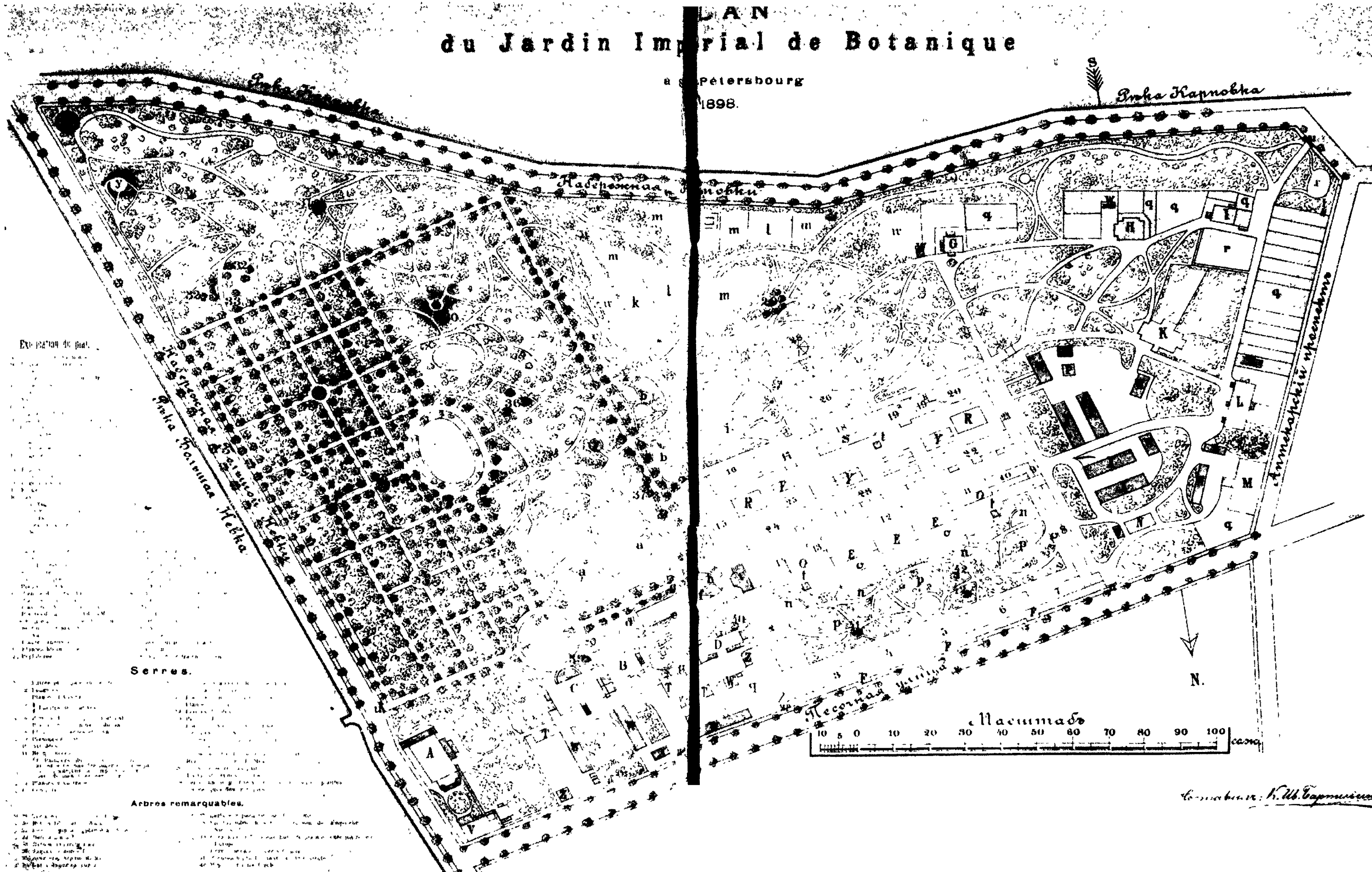
Lisovskiĭ, N., 1900: Dictionaries. (Entsikl. Slov. Brockhaus & Efron 30: 380-388.). — *A critical review of Russian and foreign encyclopaedical and special dictionaries, many of which are not included in this bibliography because they are not available in the U. S. A.*

Litvinov, D. I., 1909: A bibliography of the flora of Siberia. (Trav. Mus. Bot. Acad. Sci. St.-Pétersb. 5: i-ix, 1-458.). — *In Russian; contains over 1100 entries with indexes of authors and of geographical names.*

Liubimenko, I. I., 1937: Uchenaia korrespondentsiia Akademii Nauk XVIII veka. Nauchnoe opisanie. 1766-82. [Scientific correspondence of the Academy of Sciences in 18th century. Description for the years 1766-82]. 1-606.



PALLAS' MAP OF PRINCE DEMIDOFF'S GARDENS (cf. PLATE 53, back)



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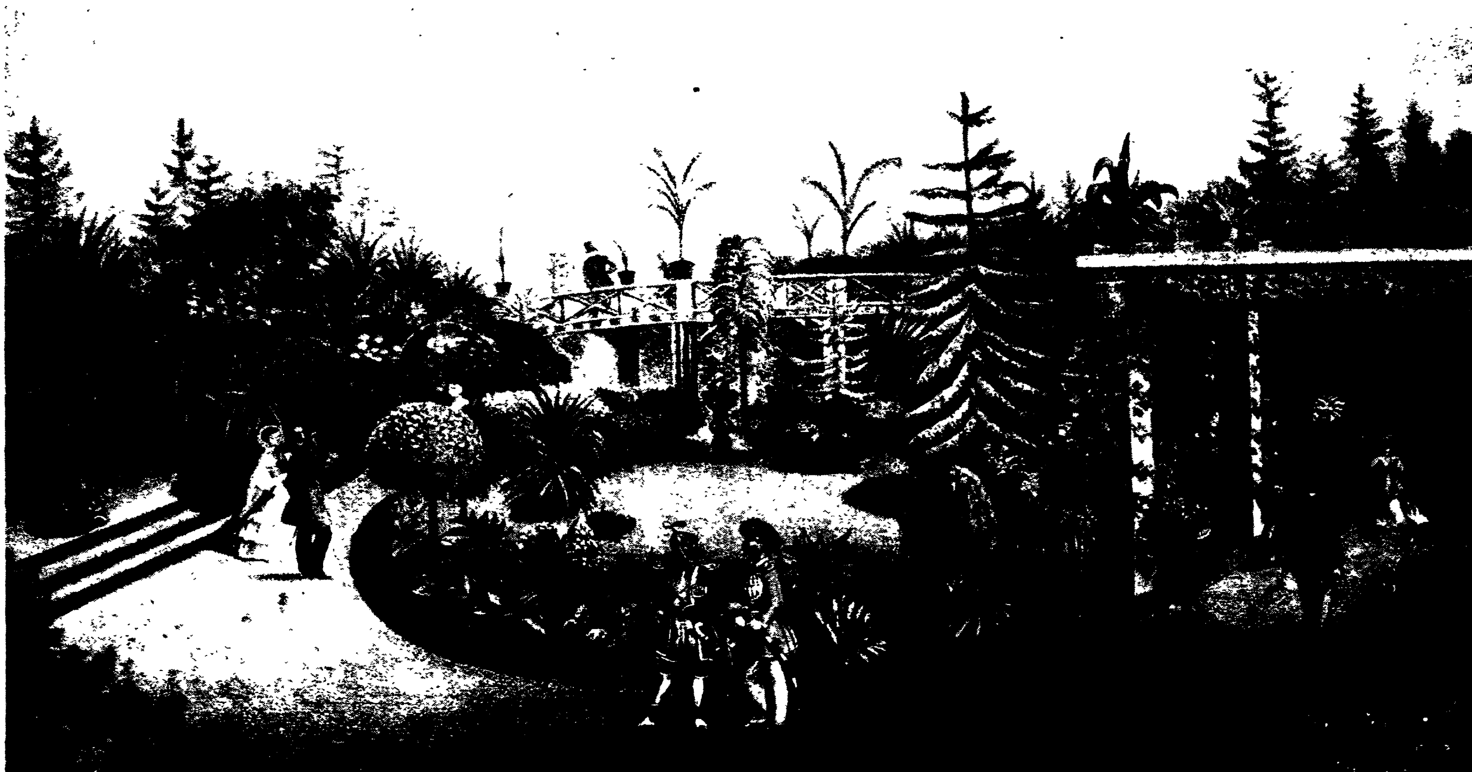

During the years of the Russian Monarchy, the Aristocracy loved horticultural shows, as the Administrators of the present generation their Agricultural Exhibits. Whereas the latter serve many purposes, the early shows were, though not only, especially noteworthy as colourful social gatherings, about which REGEL tells us so often in his *Gartenflora*. Our PLATE 56 shows one of these exhibits, the Great Flower Show of the Saint Petersburg Horticultural Society, in the Spring of 1860, drawn from the staircase near the entrance. These intriguing Victorian arrangements of flowering plants and trees drew many visitors from Society and the Army, as well as the Lions from the world of the Sciences and Arts. The Medals awarded by the Society were highly esteemed by the western European nursery establishments which contributed much material to these shows.

(Courtesy Arnold Arboretum of Harvard University)

22 photographs of the letters. 23 portraits. — *Includes letters of some prominent botanists (Pallas, Lepekhin, Gmelin, etc.) and is important for the study of botanical explorations; edited by editorial committee:* **D. S. Rozhdestvenskii, G. A. Kniazev, and L. B. Modzalewski.**

Liubimenko, V. N., 1937: Vingt ans de physiologie des plantes soviétique. (Sovietsk. Bot. 1937(5): 31-57). — *A historical review in Russian.*

Liubitskaia, L., 1914: Recherches sur les formes du Leucobryum glaucum (L) Schimp. (Bull. Jard. Bot. Pierre Grand 14: 351-419. *illus.*). — *In Russian with a French résumé; includes a bibliography of 277 entries on mosses of Russia.*

—— [Savich-Liubitskaia, L.], 1937: Le vingtième anniversaire de bryologie soviétique. (Sovietsk. Bot. 1937(5): 117-141). — *A historical account in Russian with critical review of literature and a small bibliography.*

Magakjan, A. K., 1941: Rastitel'nost' Armianskoĭ SSR. [The vegetation of the Armenian SSR]. 1-276. — *Includes an extensive bibliography of 283 entries.*

Malaiia Sovetskaia Entsiklopediia *see* Meshcheriakov, N. L.

Maleev, V. P., 1937: Recherches sur la flore et la végétation du Caucase et de la Crimée pendant les 20 ans depuis la Grande Révolution socialiste d'Octobre. (Sovietsk. Bot. 1937(5): 142-168). — *In Russian; includes data on the exploration and literature and a bibliography.*

Maleeva, O. F., 1931: The Nikita Botanical Garden under Steven (1812-24). (Jour. Bot. Gard. Nikita 17: 1-34.) — *A historical sketch in Russian with a very brief English summary.*

Manteuffel, T., 1936: Uniwersytet Warszawski w latach 1915/16-1934/35. Kronika. i-viii, 1-352. — *A section of this work — "Wydzial Matematyczno-Przyrodniczy" (Faculty of Mathematics and Natural History), pp. 213-238, includes a historical sketch of the faculty and a biographical list of its members.*

Margolina, K. F., 1934: Principal books and papers on plant industry published in the USSR in 1932. (Bull. Appl. Bot. & Pl. Breed. Ser. A.9: 173-197). — *A classified bibliographical list of Russian works without annotations.*

Markevich, A. I., 1890: Twenty fifth anniversary of the Imperial Novorossiĭsk (New Russian) University (Historical note). (Zapisk. Novorossiĭsk. Univ. 53; i-xvii, 1-734, I-XC). — *A comprehensive historical work in Russian, including sections: "agriculture", pp. 399-412 and "botany", pp. 421-441 with biographies of agriculturists and botanists.*

Maslov, S., 1850: Istoricheskoe obozrenie dieĭstviĭ i trudov Imperatorskago Moskovskago Obshchestva Sel'skago Khoziaĭstva so vremeni ego osnovaniia [1820] do 1846 goda. [Historical review of the activity and works of the Imperial Agricultural Society of Moscow from its foundation [1820] to the year 1846.] i-vi, 1-154, 1-288, 1-50, 1-16, i-viii, I-II. — *A historical survey in Russian with a bibliography of published works of the Society, pp. 145-241, including some botanical and many agricultural works.*

Matveev, Z. N., 1925: Chto chitat' o Dal'nevostochnoĭ oblasti. [What to read about the Far East]. 1-248. — *A bibliography of 3973 Russian publications on various subjects, including flora, fauna, forestry and paleontology.*

Maximov, N. A., 1943: Les progrès de la physiologie des plantes soviétique faits pendant 25 années. (Sovietsk. Bot. 1943(1): 3-14). — *A historical sketch in Russian.*

Maximowicz, C. J., 1885: Sur les collections botaniques de la Mongolie et du Tibet septentrional (Tangout) recueillies récemment par des voyageurs Russes et conservées à St. Pétersbourg. (Bull. Congr. Internat. Bot. Hort. St. Pétersb. 1884: 135-196). — *Includes some historical data on the explorations and descriptions of collections.*

—— 1889: Speech delivered at the conference of the Imperial Russian Geographical Society, November 9, 1888. (Izv. Russk. Geogr. Obshch. 24: 257-267). — *A survey in Russian of the travels and collections of N. M. Przhewalski in the Ussuri region, Tibet and Mongolia.*

Mercklin, K. E., 1848: Ueber russische Reisende und den Petersburger bot. Garten. (Bot. Zeit. 6: 463-466, 480-484). — *Includes historical data on some expeditions in European and Asiatic Russia.*

—— 1871: IX. Botanika. (Botany). (From systematical catalogue of the library of the Medico-Chirurgical Academy 1 (1): 83-137). — *Apparently a reprint from the general catalogue; not seen.*

Merrill, E. D. & Walker, E. H., 1938: A bibliography of Eastern Asiatic botany. i-xlii, 1-719. *2 maps.* — *This important bibliography includes about 500 names of Russian authors and 2300 titles of*

their works concerning a large part of Asiatic Russia: Russian Far East, eastern Siberia and some regions of Central Asia (see Subject index, geographic region 3, pp. 620-22 and map on p. 594).

Meshcheriakov, N. L., editor, 1930-31: Malaia Sovetskaia Entsiklopediia. [Small Soviet Encyclopaedia]. 10 vols. —*Contains some botanical articles and biographies of foreign botanists, but Russian botanists are not included.*

Meyer, K. I., 1940*a*: A comparative-morphological tendency in the study of higher plants at Moscow University. (Uchen Zapisk. Mosk. Gosud. Univ. 54(Biol.): 288-293. *port.*).—*Historical.*

—— 1940*b*: Botanical Garden. (Uchen. Zapisk. Mosk. Gosud. Univ. 54(Biol.): 332-339).—*A historical sketch of the development of the Botanical Garden of Moscow University.*

—— 1940*c*: Botanist Ivan Nikolaevich Gorozhankin (1848-1904) and his school. (Mosk. Obshch. Ispyt. Prir. Istor. Ser. no. 15: 1-52. *6 port.*)—*Besides the biography of* GOROZHANKIN, *it inclues data on many other botanists and their work.*

Mezhov, V. I., 1872: Bibliograficheskiĭ ukazatel' istorii russkoĭ i vseobshcheĭ slovesnosti. [Bibliographical index of the history of Russian and universal literature]. i-xxiii, 1-708.—*In Russian; this general work contains materials for a biographical dictionary of Russian writers, pp. 267-482, including many naturalists.*

—— 1878-88: Turkestanskiĭ sbornik sochineniĭ i stateĭ otnosiashchikhsia do Sredneĭ Azii voobshche i Turkestanskago Kraia v osobennosti. Tomy 1-150. Sistematicheskie i azbuchnye ukazateli . . . [Recueil du Turkestan comprenant des livres et des articles sur l'Asie Centrale en général et le Province de Turkestan en particulier. Tomes 1-150. L'indicateur systématique et alphabétique]. i-viii, 1-184. 1878; ditto for vols. 151-300: i-viii, 1-166. 1884; ditto for vols. 301-416; i-vi, 1-134. 1888. —*A systematic bibliography, including section 'Natural history' and 'Agriculture' with indexes of authors and geographical names.*

—— 1891-92: Sibirskaia bibliografiia. Ukazatel' knig i stateĭ o Sibiri na russkom iazykie i odnikh tol'ko knig na inostrannykh iazykakh za ves' period knigopechataniia. [Bibliographia sibirica. Bibliographie des livres et articles des journaux russes et étrangers, concernant la Sibérie]. Pt. 1: i-ii, 1-485. 1891: 2: i-x, 1-470. 1891; 3: i-x, 1-303, (index) 1-188. 1892.—*This bibliography in Russian contains 25,250 entries, including some botanical works; a second edition was published in 1903.*

Miansarov, M., 1874-76: Bibliographia Caucasica et Transcaucasica. Essai d'une bibliographie systématique relative au Caucase, à la Transcaucasie et aux populations de ces contrées. 1 (pts. 1-2): i-xlii, 1-804.—*A subject bibliography in Russian with a French preface: section 5, "Botanique. Zoologie," pp. 157-178, and section "Voyages," pp. 320-388, include many botanical works.*

Mikhaĭlova, M. G., 1938: Flore et végétation de la RSS d'Ukraine. Bibliographie; index des livres et des articles publiés dans les journaux. 1-61.—*A list of 1706 titles with Ukrainian and French prefaces.*

Mishchenko, P. I., Jaczewski, A. A., et al., 1913-28: Section of referates and bibliography with a subsection of phytopathology and mycology. (Bull. Appl. Bot. & Pl. Breed. Vol. 6-19).—*This section of the Bulletin published either as a special number in some volumes or at the end of each fascicle includes referates of current Russian and foreign works and indexes of current literature on applied botany in Russia.*

Modzalewski, B. L., 1908: Spisok chlenov Imperatorskoĭ Akademii Nauk, 1725-1907. [List of the members of the Academy of Sciences, 1725-1907]. i-viii, 1-404. *9 silhouettes.*—*Gives short biographical data on many botanists, members of the Academy; for continuation see next entry.*

—— 1925: Spisok deĭstvitel'nykh chlenov Akademii Nauk Soiuza Sovetskikh Sotsialisticheskikh Respublik, 1725-1925. [A list of the active members of the Academy of Sciences of USSR]. 1-35.—*A continuation of the preceding entry.*

—— *see also* **Liubimenko, I. I.**

Monteil, R., 1891: Öfversigt af forstlitteraturen i Finland till år 1890, innefattande sådana böcker, afhandlingar, artiklar, författningar m. m., som äro af forstligt intresse. [A survey of Finnish literature on forestry up to the year 1890, including such books, treatises, articles, statuses, etc., which may have a bearing on forestry.] (Bilaga F. Forstfören. Medd. 8: 1-52).—*A bibliography of Finnish literature on forestry of Finland.*

Moskovskoe Obshchestvo Ispytateleĭ Prirody, 1940-1941: Seriia istoricheskaia. [Historical series]. Numbers 1-17.?—

A commemorative publication of this oldest Russian society of naturalists (1805-1940) consisting of biographical sketches of prominent deceased members, including some botanists and explorers; seventeen numbers have been seen.

Murashkinskiĭ, K. E., 1927: History and perspectives of the study of mycological flora of Sibiria. (Asia 1927(3): 68-75). — *A historical sketch with data on the expeditions and collections.*

Nauka i nauchnye rabotniki SSSR. Spravochnik sostavlennyĭ komissieĭ "Nauka i nauchnye rabotniki SSSR." [Science and scientific workers of USSR. A guide book compiled by the commission "Science and scientific workers of USSR."] Editors **S. F. Oldenburg** and **E. F. Karsky**; 1926-1934: Pt. 1: not seen; pt. 2: Scientific institutions of Leningrad, i-viii, 1-407, *port.* 1926; pt. 3: not seen; pt. 4: Scientific workers of Moscow, i-xx, 1-570, 1-62. 1930; pt. 5: Scientific workers of Leningrad, i-xx, 1-723, *port.* 1934; pt. 6: Scientific workers of USSR excluding those of Moscow and Leningrad, i-viii, 1-810. 1928. — *Volumes 4-6 are very important sources of information on living scientists of Russia and may be compared with "American men of science"; the whole work badly needs a new edition.*

Nazarov, M. I., 1926: Herbarium vivum Universitatis Mosquensis. (Bull. Jard. Bot. Princ. U.R.S.S. 25: 266-314). — *In Russian; includes a historical sketch, description of collections and many biographical references.*

Nekrasova, V. L., 1945:: Contribution à l'histoire du Jardin Botanique de l'Académie des Sciences. (Sovietsk. Bot. 13(2): 13-37). — *A historical sketch in Russian including brief biographies of several botanists and explorers.*

Nikolaĭ Mikhaĭlovich, Grand Duke, 1907-08: Moskovskiĭ nekropol'. [Moscow necropolis]. 1: i-xxiii, 1-517. 1907; 2: 1-486. 1908; 3: 1-432. 1908. — *In Russian; includes data on many naturalists.*

— — 1914: Russkiĭ provintsial'nyĭ nekropol'. [Russian provincial necropolis]. 1: ix, 1-1008. —*Includes data on deceased persons of 12 northern and central provinces of European Russia.*

Novikov, N., 1772: Opyt istoricheskago slovaria o rossiĭskikh pisateliakh. [An essay on a historical dictionary of Russian·writers]. [1-2], [1-6], [1-5], 1-264. Reprinted in Efremov, P. A., 1867: Materialy dlia istorii russkoĭ literatury, pp. 1-128. — *Includes biographical and bibliographical data on some botanists.*

Novombergskiĭ, N., 1907: Vrachebnoe stroenie v do-Petrovskoĭ Rossii. [Progress of medicine in Russia before the time of Peter the Great]. i-vii, 1-387, i-xcix, i-v and 4 volumes of "Prilozheniia" (Supplements). — *In Russian; pp. 118-166 include historical data on the development of medical and pharmaceutical botany.*

Obruchev, V. A., 1897: Kurze Uebersicht der von der Kaiserlich-Russischen Geographischen Gesellschaft ausgerüsteten Expeditionen zur Erforschung Asiens. (Izv. Vost.-Sibirsk. Otd. Russk. Geogr. Obshch. 27(1): 1-40). — *In Russian; includes data on botanical activity of various expeditions in Asia.*

Ohl, I. A. & Domrachev, G. V., 1933-41: Soviet botanical literature for the year 1930. (Sovietsk. Bot. 1933 (1): 92-127; (ditto for 1931) 1933 (3-4): 272-304; (ditto for 1932) 1933 (6): 148-192; (ditto for 1933) 1935 (1): 154-204; (ditto for 1934) 1936 (3): 135-197; (ditto for 1935) 1937 (2): 153-238; (ditto for 1936) 1940 (1): 110-142; 1940 (4): 84-102; 1941 (1-2): 196-221; (ditto for 1937) 1941 (4): 142-178; 1941 (5-6): 111-141). — *A classified list of current botanical literature.*

Ohl, L. A., *see* **Elenkin, A. A.**

Oldenburg, S. F., *see* **Nauka.**

(S.) **Orgelbranda Encyklopedja Powszechna**, 1898-1912. 18 vol. — *A second edition of "Encyklopedja Powszechna" (first edition has not been seen) including biographies of many Polish naturalists and explorers.*

Osipov, I. P. & Bagalieĭ, D. I., 1908: Fiziko-matematicheskiĭ fakul'tet Khar'kovskago Universiteta za pervyia sto liet ego sushchestvovaniia (1805-1905). [Phys.-Mathematical faculty of Kharkov University for the first hundred years of its existence. (1805-1905)]. i-vi, 1-357, 1-248, I-XIV. *90 port.* — *Includes a historical sketch of the faculty and a biographical dictionary, pp. 1-248, with biographies of many botanists.*

Palibin, I. V., 1898: Imperial St. Petersburg Botanical Garden and its past. (Nauchnoe Obozrenie 1898(8): 1327-1342). — *A historical survey in Russian.*

— — 1928: Short history of the tea industry in Georgia and Abchasia. (Bull. Appl. Bot. & Pl. Breed. 18(3): 247-272.). — ***In Russian with an English résumé; includes historical data on the cultivation and introduction of the tea plant.***

Palimpsestov, I., 1855: Otchet o dieiatel'nosti Imper. Obshch. Sel'sk. Khoz. za 25 liet. [Report on the activity of the Imper. Agricult. Soc. for 25 years.] — *In Russian; not seen.*

Pavlov, N. V., 1938: Flora Tsentral'nogo Kazakhstana. Ch. 3. Dvudol'nye: Spaïnolepestnye. [Flora of Central Kazakstan. Pt. 3. Dicotyledones: Sympetalae]. (Trudy Kazakhst. Fil. Akad. Nauk SSSR fasc. 17: 1-429). — *First and second parts of this work have not been seen; part 3 contains "A sketch of botanical exploration of Central Kazakstan", pp. 398-423.*

— — 1940: Rastitel'nost' Kazakhstana. Vol. 2. Literaturnye istochniki po flore i rastitel'nosti Kazakhstana. [Vegetation of Kazakstan. Vol. 2. Literature sources of the flora and vegetation of Kazakstan.]. (Trudy Kazakhst. Fil. Akad. Nauk SSSR fasc. 19: 1-182). — *A comprehensive bibliography of 2337 entries, including at least 700-800 works having very slight, if any, relation to Kazakstan and even to botany (climatology, geology, anthropology, etc.); "Adjoining regions" (see Subj. index, pt. 14) include, for instance, Arabia, West Europe and even "the globe".*

Pawlowski, B., 1927: Rozwój florystyki i systematyki roślin w Polsce w latach 1872-1925. (Le développement de la floristique et de la systématique des plantes en Pologne en 1872-1925). — *In Polish; not seen.*

Pekarsky, P. P., 1862: Nauka i literatura v Rossii pri Petrie Velikom. [Science and literature in Russia in the time of Peter the Great]. 1: [1-6], i-vi, 1-578. 1862; 2: i-ii, 1-694, i-xxv. 1862. — *In Russian; volume 1 includes data on the establishment of the Academy of Sciences, medical gardens and pharmacies.*

— — 1870-73: Istoriia Imper. Akademii Nauk v Peterburgie. [History of the Academy of Sciences of St.-Petersburg]. 1: i-lxviii, 1-774. 1870; 2: i-lviii, 1-1042. 1873. — *First volume of this work includes biographies of botanists, members of the Academy.*

Petrov, F. N., edit., 1927: Desiat' let sovetskoï nauki. 1917-27. [Ten years of Soviet science]. 1-479, *illus.* — *A historical sketch, including data on development of botany (plant physiology, genetics, etc.) in section "biology," pp. 307-434.*

Petrov, V. A., 1940: Early stages of the development of botany at Moscow University. (Uchen. Zapisk. Mosk. Gosud. Univer. 54(Biol.): 259-268. *port.*). — *Historical.*

Petrushevsky, F. F., *see* **Brockhaus, F. A. & Efron, I. A.**

Pietukhov, E. V., 1902: Imperatorskiï, Iur'evskiï, byvshiï Derptskiï, Universitet za sto liet ego sushchestvovaniia (1802-1902). [Imperial Jurjev, formerly Dorpat, University for one hundred years of its existence (1802-1902)]. 1(1802-1865): i-iv, 1-620. 1902; Prilozhenie-Statisticheskiia tablitsy. [Supplement-Statistical tables]. 1-39. 1902. — *An unfinished historical survey; includes a few data on the history of botany.*

Pliushkin, L. N., *see* **Akademiia Nauk**, 1930.

Pohle, R., 1915: Ukazatel' liesovodstvennoï i botaniko-geograficheskoï literatury Sievernoï Rossii i Finliandii. [Guide to the literature on forestry and geobotany of Northern Russia and Finland]. — *In Russian; not seen.*

Polianskaia, O. S., 1937: Revue des travaux sur la végétation de la péninsule de Kola apparus en 1935-36. (Sovietsk. Bot. 1937(5): 227-234). — *An account in Russian of the study of the flora since 1917 with a small bibliography of 22 entries.*

Polnaia Entsiklopediia russkago sel'skago khoziaïstva *see* **Devrien, A. F.**

Polovtsov, A. A., editor, 1896-1918: Russkiï biograficheskiï slovar'. [Russian biographical dictionary]. 25 volumes. — *Edited by the Imp. Russian Historical Society under supervision of its president, Prof.* A. A. Polovtsov; *includes important data on Russian botanists and their explorations. Some letters of the alphabet are omitted and the volumes are not numbered.*

Polski slownik biograficzny (Polish biographical dictionary) 1: i-xvi, 1-479. 1935; 2: i-vi, 1-479. 1936; 3: i-xv, 1-479. 1937; 4: i-xv, 1-480. 1938; 5: 1-384. 1939. — *An unfinished work (letters A-D); includes biographies of Polish botanists.*

Popov, V. M., 1927: Agronomo-botanical explorations in Amutr region in the past. (Proizv. Sily Dal'n Vost. 3: 337-353). — *Includes data on the explorers and their travels.*

Prozorovskiï, A. V., 1937: L'étude géobotanique des demi-déserts, des déserts et des montagnes de l'Asie Moyenne pendant les derniers 20 ans. (Sovietsk. Bot. 1937(5): 169-188). — *A summary in Russian.*

— — 1940: Semi-deserts and deserts of USSR. (Vegetatio URSS 2: 267-480.

f. 1-43).—In Russian; includes a historical sketch of steppe exploration and a bibliography, pp. 471-480.

Rabotnov, T. A., 1936: Revue de travaux concernants la végétation de la RSSA Jacoutienne. (Journ. Bot. URSS 21: *727-738. 2 port.).—A historical sketch in Russian of the botanical study of Yakoutia for 200 (1736-1936) years with a bibliography.*

Radde, G. I., 1891: Kratkiĭ ocherk istorii razvitiia Kavkazskago Muzeia v pervye 25 liet ego sushchestvovaniia s 1-go ianvaria 1867 po 1 ianvaria 1892. [Kurze Geschichte der Entwickelung des Kaukasischen Museums während der ersten 25 Jahre seines Bestehens, 1 Januar 1867 bis 1 Januar 1892]. 1-65. *Plan.—A historical sketch published separately in Russian and German.*

——Editor, 1901: Kollektsii Kavkazskago Muzeia obrabotannyia sovmiestno s uchenymi spetsialistami i izdannyia D-rom **G. I. Radde.** [Die Sammlungen des Kaukasischen Museums im Vereine mit Special-Gelehrten bearbeitet, und herausgegeben von **Dr. Gustav Radde**]. Band 2: Botanik. [1-10], i-x, 1-101, [1-4], 1-201. *12 port. 20 pl. 3 maps.—In Russian with some parts in German; besides description of collections and a sketch of the vegetation, it includes data on and itineraries of author's travels from 1864 to 1894.*

Ramzaev, D. (*editor*) **and others**, 1935: Saratovskiĭ Gosudarstvennyĭ Universitet im. Chernyshevskogo, 1909-1934. [Chernyshevsky State University at Saratov, 1909-1934]. 1-94. *illus.—In Russian, a commemorative historical work, containing brief data on the progress of plant anatomy, pl. physiology and geobotany with a small bibliography.*

Recke, J. F., von & Napiersky, C. E., 1827-61: Allgemeines Schriftsteller—und Gelehrten—Lexikon der Provinzen Livland, Esthland und Kurland. 1 (A-F): i-xvi, 1-624. [1-2]. 1827; 2 (G-K): 1-620. 1829; 3 (L-R): 1-598. 1831; 4 (S-Z): 1-628. 1832; Nachträge und Forsetzungen. 1 (A-K): [1-4], 1-352. 1859; 2 (L-Z): 1-287, 1-24. 1861.—*Includes biographies of many botanists; 'Nachträge und Forsetzungen' by* **Napiersky, C. E. & Beise, T.**

Regel, C. (Regelis, K.), 1931-39: Fontes florae Lituanae.—Lietuvos floros šaltiniai. I. (Script. Hort. Bot. Univ. Vytauti Magni (Mat.—Gamtos Fakult. Darbu 5:—) 1: 221-289; II (1932) *ibid.* 2: 3-71. *3 pl.;* III (1935) *ibid.* 3: 87-128; IV (1936) *ibid.* 4: 47-81; V. (1937) *ibid.* 5: 67-84; VI (1939) *ibid.* 6: 5-27.—*Includes "Bibliographia botanica Lituana" with critical review of the literature and data on herbaria.*

Regel, R. E., 1915*a*: L'organisation et les travaux du Bureau de botanique appliquée pendant vingt ans de son fonctionnement (1894-1914). (Bull. Appl. Bot. & Pl. Breed. 8: *327-723).—In Russian with an extensive French résumé, pp. 659-723; a comprehensive historical work with a critical review of the activity of the Bureau and a classified bibliography of works published by it.*

——1915*b*: Les travaux du Bureau dans les branches spéciales et les résultats obtenus. (*ibid.* 8: 1465-1637).—*An abridged French translation of the chapter 4 of the preceding article. See also* **Dokuchaev, V. V.**

Renvall R. A., 1869: Biografiska anteckningar öfver det Finska Universitetets lärare, embets- och tjenstemän, från dess flyttning till Helsingfors år 1828 till nuvarande tid. [Biographical notes about the Finnish University's teachers, professional men and civil servants, from the time of its removal to Helsinki in 1828 and up till the present]. i-xxiv, 1-246.—*A biographical dictionary.*

——1891: Andra tillökade uppl. Finlands Universitet 1828-1890. Biografiska uppgifter öfver dess lärare, ämbets- och tjänstemän. [Second enlarged edition. Finnish University 1828-1890. Biographical data concerning its teachers, professional men and public servants]. i-xxxi, 1-434.

Reuter, E., 1944: Index generalis serierum Notiser I-XIV (1848-1875) et Meddelanden 1-50 (1876-1925) Societatis pro Fauna et Flora Fennica. (Acta Soc. Fauna & Fl. Fenn. 64: i-xv, 1-676).—*A comprehensive work, including a bibliography, pp. 1-176 and "Pars botanica. Index systematicus", pp. 177-463.*

Richter, W. M., 1813-19: Geschichte der Medicin in Russland. 1: i-xxiii, 1-457. 1813; 2: i-xxxii, 1-440, 1-178. 1815; 3: i-xxxii, 1-629. 1819.—*Volume 3 of this work contains many biographies of physicians-botanists.*

——1814-20: Istoriia meditsiny v Rossii. [History of medicine in Russia]. 1: i-xx, 1-440. 1814; 2: i-xviii, 1-352, *prib.* 1-16. 1820; 3: i-xxxii, 1-544, *prib.* 1-112. 1820.—*A Russian translation of the preceding.*

Rogovich, A. S., 1864: Historical notes on the botanical garden of the University of St. Vladimir. (Universit. Izv. [Kiev] 1864 (4): 1-25; 1864 (5): 34-50).— ***Not seen. Reprint 1-42.***

—— 1875: Bibliographical index of works on the natural history in the limits of Kiev scholar region: Volhynia, Podolia, Kiev, Poltava and Chernigov governments. (Zapisk. Iugo-Zap. Otd. Russk. Geogr. Obshch. 2: 275-287). — *Includes a list of botanical works, pp. 281-85; not seen.*

Rogozhin, V. N., *see* **Sopikov, V. S.**

Rossiïskaia Akademiia, *see* **Sukhomlinov, M. I.**

Rozhdestvenskiï, D. S., *see* **Liubimenko, I. I.**

Rudzki, A., 1852: Index of the articles published in the Journal of Forestry (Liesnoï Zhurnal) from 1833 to 1851 which have some importance at the present time. (Gazeta Liesovodstva i Okhoty, 1852: no. 11, 15, 17, 18). — *Not seen. See also* **Verekha, P.**

Ruoff, S., 1926: Zusammenstellung der russischen Moorliteratur für die Jahre 1914-1926. (Geol. Arch. 1926 (4): 103-108). — *Not seen; for continuation see next entry.*

—— 1936: Revue de la littérature russe sur les marais et la tourbe, apparu de 1925 à 1934. (Sovietsk. Bot. 1936 (4): 176-204). — *A subject bibliography in Russian of 590 entries.*

Ruprecht, F. J., 1864: Zur Geschichte der Museen der Kaiserlich. Akademie der Wissenschaften. I. Das botanische Museum. (Bull. Acad. Sci. St. Pétersb. 7: Suppl. 2: 1-10). — *A historical sketch with data on collection; published also in Russian (Zapisk. Akad. Nauk. 5: 139-162. 1864).*

—— 1865a: Materialy dlia istorii Imperatorskoï Akademii Nauk po chasti botaniki. [History of the Imperial Academy of Sciences, section of botany]. (Zapisk. Akad. Nauk 7: Suppl. 3: 1-35). — *Includes historical data and description of collections; for a shorter German version see next entry.*

—— 1865b: Beiträge zur Geschichte der Kaiserlichen Akademie der Wissenschaften. Botanik. (Bull. Acad. Sci. St. Pétersb. 8: Suppl. 1: 1-15). Reprinted (Mél. Biol. Acad. Sci. St. Pétersb. 5: 57-98. 1865).

Russkiï Biograficheskiï Slovar', *see* **Polovtsov, A. A.**

Russkoe Geograficheskoe Obshchestvo (Russian Geographical Society), 1886-96: Ukazatel' k izdaniiam Imper. Russkago Geograficheskago Obshchestva i ego otdielov. [Guide to publications of the Imper. Russian Geographical Society and its branches from 1846 to 1875]: 1-144, 1-33. 1886; ditto 1876-85: 1-75, 1-20. 1887; ditto 1886-95: i-ii, 1-190, 1-27. 1896. — *A list of publications with tables of contents for each volume, including some botanical works and accounts of botanical explorations, with indexes of the authors and geographical names.*

R-v, V., 1902: Universities. (Entsiklopedicheskiï Slovar' Brockhaus & Efron 34: 751-803). — *Includes historical data on Russian universities, pp. 788-800, and a bibliography; see also the same dictionary for the history of every university under its name.*

Sabanin, D. A., *see* **Shaposhnikov, V. N.**

Sacklén, J. F., 1822-35: Sveriges Läkare-Historia ifrån Konung Gustaf I:s till närvarande tid. [Sweden's medical history from the time of King Gustav I until the present]. 1: 1-920. 1822; 2(1): 1-764. 1823; 2(2): 1-574. 1824; Suppl. 1-664. 1835. — *Includes some data on Finland.*

—— 1833: Sveriges Apothekare-Historia, ifrån Konung Gustaf I:s till närvarande tid. Ett bihang till Sveriges Lakare-Historia. [The history of Pharmacology in Sweden, from the time of King Gustaf Ist until the present. An appendix to Sweden's Medical history).] 1-480. — *Includes some historical and biographical data on Finland.*

Saelan, A. T., 1867: : Öfversigt af Finlands botaniska litteratur. (Notis. Sällsk. Fauna & Fl. Fenn. Förh. VII. ny ser. IV. 1867: 83-121). — *A bibliography of the literature on the botany of Finland.*

—— 1871: Finsk botanisk litteratur år 1866-1870. (Bot. Notis. 1871: 160-162, 193-195).

—— 1916: Finlands botaniska litteratur till och med år 1900. (Act. Soc. Fauna & Fl. Fenn. 43(1): i-xi, 1-633). — *A comprehensive bibliography of botanical works on Finland, indispensable for the study of the history of botany of that former part of the Russian Empire.*

St.-Petersburg Institute of Forestry, 1903: Istoricheskiï ocherk razvitiia S.-Peterburgskago Liesnogo Instituta. [Historical sketch of the development of the St.-Petersburg Institute of Forestry]. 1803-1903. [14], 1-193. *21 pl. (port.). 35 pl. 23 f.* Supplement. 1-157. — *Includes historical and biographical data; see also* **Borodin, I. P.**, *1905.*

Saitov, V., 1912-13: Peterburgskiï nekropol'. [St. Petersburg necropolis]. 1: i-xx, 1-715. 1912; 2: 1-726. 1912; 3: 1-649. 1912; 4: 1-747. 1913. — *In Russian; one of the most reliable sources of information on the dates of birth and death of many naturalists.*

Satsyperov, F. A., 1917: Medicinal plants

of Russia. (Bull. Appl. Bot. & Pl. Breed. 10(Suppl. 17): 1-110) —*Includes a bibliography of works in Russian, pp. 91-100, and foreign languages, pp. 100-110.*

Schischkin, B. K., 1927: Results of the study of the vegetation of Siberia for 200 years. (First Congress for research study of Siberia: 186-194). —*Not seen.*

— — 1929: Botanical exploration of Siberia. (Sibir. Sov. Entsikl. 1: 383-387). —*A brief historical survey in Russian.*

— — 1942: Taxonomie et floristique en URSS pendant 25 ans. (Sovietsk. Bot. 1942(6): 3-11). —*A historical sketch in Russian of the progress of botany and floristics from 1917 to 1942.*

— — 1945: Institut botanique Komarov de l'Académie des Sciences d'URSS. (Sovietsk. Bot. 13(2): 5-12). —*A historical note in Russian compiled on account of 220th anniversary of the Academy of Sciences USSR.*

Schischkin, I. K., 1930· Contributions to the flora of the basin of Iman River. (Zapisk. Vladiv. Otd. Russk. Geogr. Obshch. 22(2): 5-173. *map*). —*Includes data on the exploration of this region of the Far East and biographical notes on the explorers.*

Schmidt, O. J., editor, 1926-39: Bol'shaia Sovetskaia Entsiklopediia. [Great Soviet Encyclopaedia]. 65 volumes. —*Includes several articles on botany of Russia and biographies of Russian and foreign botanists; a few volumes have not yet been published or did not yet reach the U. S. A.*

Semenov, P. P. & Dostoevsky, A. A., 1896: Istoriia poluviekovoĭ dieiatel'nosti Imp. Russkago Geograficheskago Obshchestva, 1845-1895. [History of 50 years of activity of the Russian Geographical Society from 1845 to 1895]. 1: i-xxx, 1-470, *illustr.*; 2: i-xi, 471-980 *illus.*; 3: i-viii, 981-1378, index 1-66, 1-7. *illus.* —*Contains detailed data on many botanical expeditions with portraits of many prominent explorers.* See next entry.

Semenov, V. P. and P. P. & Lamansky, V. I., editors, 1899-1914: Rossiia. Polnoe geograficheskoe opisanie nashego otechestva. [Russia. A complete geographical description of our mother country]. 11 vols. (1-3, 5-7, 9, 14, 16, 18-19). —*A very valuable source of information not only on geography but also on natural history (flora, fauna, agriculture, horticulture, etc.) of Russia; arranged by regions and includes subjects and geographical indexes, bibliographies, maps and illustrations.*

Semenov, V. F., 1927: The fifty years of existence of the Western Siberian Section of Russian Geographical Society in Omsk. (Zapisk. Zap. — Sibirsk. Otd. Russk. Geogr. Obshch. 39: 1-145. 1 map). —*In Russian with an English summary; contains data on exploration work of the society in western and eastern Siberia, and a bibliography of 606 entries.*

Serebriakov, K. K., 1941: Ocherki po istorii botaniki. Chast' 1. [Sketches of the history of botany. Part 1]. —*Includes data on the development of plant anatomy and physiology in Russia; not seen.*

Serebrovskiĭ, A. S., 1940: Department of Genetics. [at Moscow University]. (Uchen. Zapisk. Mosk Gosud. Univ. 54(Biol.): 166-175. *port.*). —*A survey of the progress of genetics in the fields of zoology and botany.*

Shaposhnikov, V. N., 1940: Microbiology at Moscow University. (Uchen. Zapisk. Mosk. Gosud. Univ. 54(Biol.): 322-324). —*Historical.*

— — **& Sabanin, D. A.**, 1940: Plant physiology at Moscow University. (Uchen Zapisk. Mosk. Gosud. Univ. 54(Biol.): 305-314. *port.*). —*Historical.*

Shennikov, A. P., 1937: Vingt ans de géobotanique théorique. (Sovietsk. Bot. 1937(5): 58-94). —*A critical analysis in Russian of the progress of geobotany in USSR.*

— — 1938: Meadow vegetation of the USSR. (Vegetatio URSS 1: 429-647. *f. 1-50*). —*In Russian; includes a bibliography of 250 entries.*

Shevyrev, S., 1855: Istoriia Imperatorskago Moskovskago Universiteta . . . 1755-1855. [History of the Imperial Moscow University . . . 1755-1855]. i-xii, 1-581. [1-3]. —*Includes some data on the Botanical Department and Museum of Natural History and biographical material.*

Shlykov, G. N., 1936: Plant introduction. i-iv, 1-503. *f. 1-97.* —*A comprehensive work in Russian on the introduction of plants into the U.S.S.R.; includes 12 pages of bibliography.*

Sibirskaia Sovetskaia Entsiklopediia [Siberian Soviet Cyclopaedia], 1929-1932: 1: i-xxx, 1-938. *illus.* 1929; 2: 1-1152. *illus.* 1931; 3: 1-804. *illus.* 1932. —*Includes data on botany and botanical explorations and biographical sketches of many botanists and explorers; supposed to be in 4 volumes, but probably only 3 volumes have been published.*

Skvortzov, B. V., 1919-21: The bibliography on the botany, zoology, and the rural economy of Manchuria. First

part. (Journ. N. China Branch Roy. Asiat. Soc. 50: 104-107. 1919; (Second part) *ibid.* 52: 104-111. 1921. — *Includes some Russian works.*

— — 1928: Botanical explorations in Kirin and Hei-Lungkiang provinces from 1886 to 1923. (Obshch. Izuch. Manchzh. Kraia 1928(7): 62-66. — *Includes historical sketch and a bibliography not seen.*

Slovar' Kavkazskikh dieiateleĭ, 1890. [Dictionary of Caucasian biography.]. — *A general biographical dictionary of Caucasus region, including biographies of some botanists; not seen.*

Smirnov, P. A., 1940: Herbarium of Moscow University. (Uchen. Zapisk. Mosk. Gosud. Univ. 54(Biol.): 325-332). — *A historical sketch.*

Sobachkov, V., 1864: Brief review of horticulture in Moscow before the time of Peter the Great. (Zhurn. Sadov. Obshch. Liubit. Sadov. 1864(4): 70-84). — *In Russian; not seen.*

Société des naturalistes de Moscou, 1936: Tables des matières contenues dans les éditions de la Société des naturalistes de Moscou (1805-1934). Séconde partie. 1-112. Supplément, 1-4. — *A classified list of articles, including "Paléontologie," pp. 38-45, "Botanique," pp. 45-47, 84-88, 97-98 and Suppl. 1-2.*

— — *see also* **Ballion, E.**, and **Lipschitz, S. J.**

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— — 1900-17: Istochniki slovaria russkikh pisateleĭ. [Sources of the dictionary of Russian writers]. 1: i-vi, 1-814. 1900; 2: 1-598. 1910; 3: 1-524. 1914; 4: 1-542. 1917. — *An unfinished work in Russian (letters A-N) containing very brief biographical data on writers and reviews of their works; includes many naturalists and explorers.*

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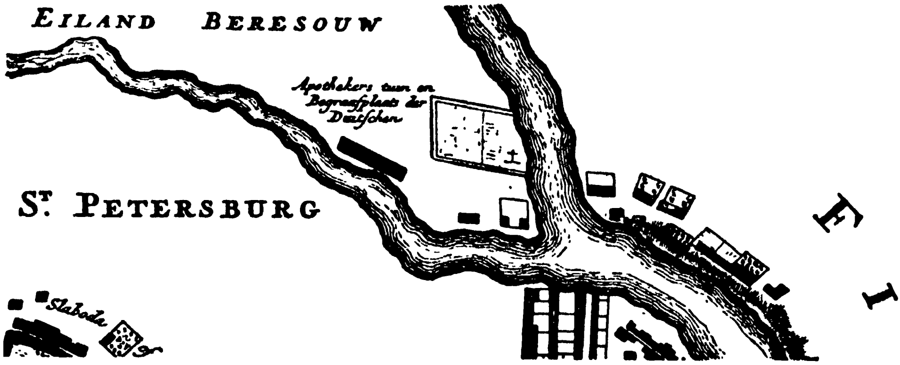
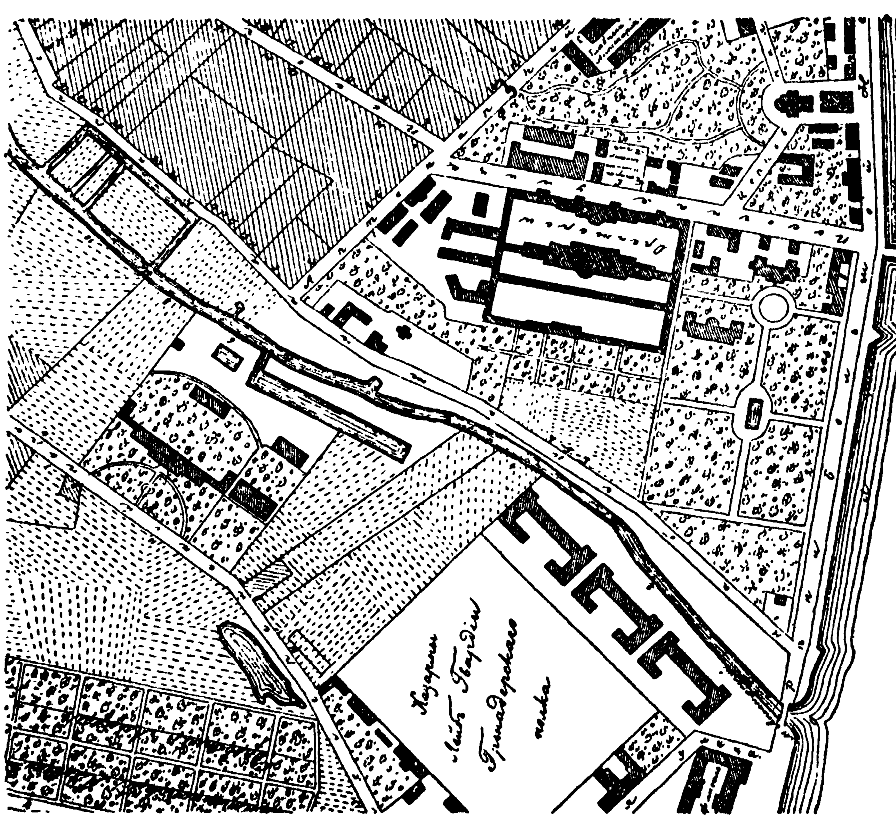
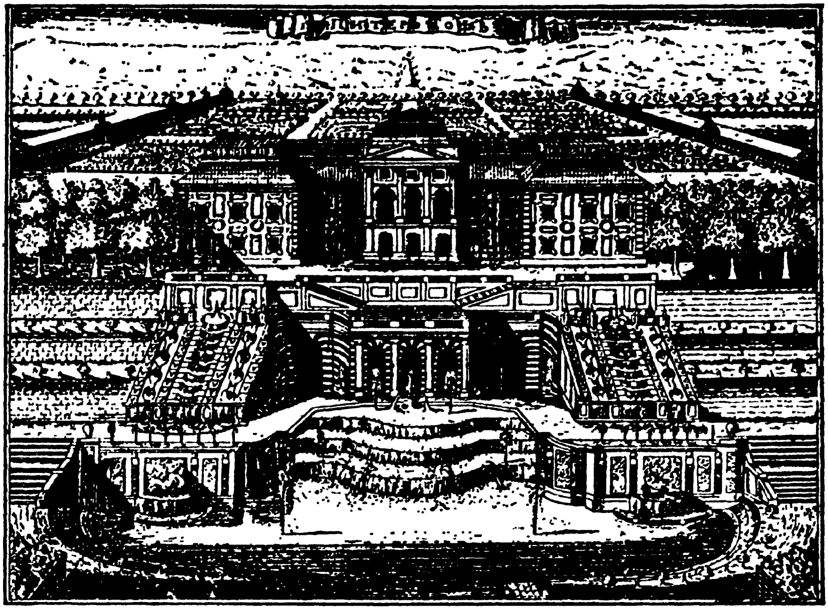


PETER THE GREAT, who visited Europe, particularly the Netherlands, during 1697 and 1698, was much impressed by the state of horticulture and botany in W. Europe. After his return, an Apothecaries Garden was established near the German cemetery, at the banks of the Bol'shaia Nevka, the location and early extent (ca. 1727-1730) of which we find on an old Dutch map (from OTTEN's description of St. Petersburg, *supra*). At the middle of the past century (in the 1860's) the garden had developed greatly, greenhouses and other buildings (*infra*) had been erected along the Pesochnaja ulitsa. For a plan of these famous and influential gardens in 1899 *vide* PLATE 55 (the best history of the gardens will be found in FISCHER VON WALDHEIM's sumptuous three-volume history, published in 1913/15 at the occasion of the gardens' bicentenary, described by Captain ASMOUS on page 96).





The Cascade and Upper Garden at Peterhof (*cf.* pl. 53, *back*) which have, unfortunately, been damaged greatly during the recent siege of Leningrad.

SUBJECT INDEX

Section 1 — Bibliographies

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FACTORS *in* BOTANICAL PUBLICATION *and other* ESSAYS

Neil E Stevens

FACTORS

in

BOTANICAL PUBLICATION

and

OTHER ESSAYS

by

NEIL E. STEVENS, Ph.D.

Professor of Plant Pathology, University of Illinois; formerly Forest Pathologist and Senior Pathologist, Bureau of Plant Industry, U.S.D.A.; Professor of Botany, Univ. of Illinois; and Senior Cranberry Specialist, Wisconsin State Dept. of Agriculture

NEIL E. STEVENS was born in Portland, Maine, April 6, 1887, the son of THOMAS JEFFERSON and HATTIE (MANTLE) STEVENS. Ten years later the family moved to Auburn, Maine. In this city his ancestors were in the small group of first settlers. He was graduated from the Edward Little High School of Auburn in 1904, from Bates College in 1908, and received the Ph.D. degree from Yale in 1911.

In 1914 he married MAUDE BRADFORD, also a native of Maine. They have three children, twin sons, RUSSELL BRADFORD (Ph.D. Wisconsin), now on the staff of the Botany Department at the University of Tennessee, CARL MANTLE, 2nd (Ph.D. Illinois), now on the staff of the Chemistry Department of the State College of Washington, and a daughter, MARY CHRISTINE, now on the research staff of Messrs. Bauer and Black, Chicago.

After a year of teaching at Kansas State College, he entered the Bureau of Plant Industry, in 1912, as Forest Pathologist. He continued in this bureau for a little more than 23 years, being assigned to four different divisions with corresponding changes in projects. The longest single assignment, lasting 15 years, was spent in the Division of Fruit Diseases. During this period he was for seven years (1931-1936) adjunct Professor at George Washington University, teaching general botany and the history of botany, and for three years secretary and director of the Arlington and Fairfax Building and Loan Association. Since February, 1936, he has been Professor of Plant Pathology (or Botany) at the University of Illinois. During nine years, beginning in 1937, the summer season was spent in field work on the cultivated Cranberry, making in all 24 seasons spent in the study of this uniquely American crop.

In 1930 and again in 1935 he was an official delegate to the International Botanical Congresses in England and Holland. The following offices in professional societies have been held: Botanical Society of Washington: secretary 1927, president 1931; The American Phytopathological Society: vice-president 1933, president 1934; The Mycological Society of America: member of the council 1932, vice-president 1944; A.A.A.S., vice-president and chairman of section G 1939; Botanical Society of America: vice-president 1940 and president in 1946. Since 1943 he has been a member of the division of Biology and Agriculture of the National Research Council. Since its founding in 1935 he has been an advisory editor of the Botanical Review.

EDITOR'S FOREWORD

We have much pleasure in presenting, in this special issue of CHRONICA BOTANICA, *a collection of the general writings of Dr.* NEIL EVERETT STEVENS, *distinguished American biologist and biological essayist. These essays, he told us, were written "as the spirit moved" over a period of more than a quarter of a century. Many of them are of international importance and they are all of an unusual permanent interest. Altogether they represent something less than half of Dr.* STEVENS' *published papers of this type. Some such as, "The Excessive Politeness of American Botanists" and "The Full Importance of Photosynthesis" have been omitted because they were actually joint papers. Others were too obviously dated or of a decided local interest. In the latter class comes a group of papers under the general title, "Some Cranberry Growers I Have Known." This was a series of character sketches of men the author had come to know and appreciate during his field work, a group of unusual men dealing with an unusual crop.*

Some of us feel that material which has once been published should not be reprinted unless the original publication(s) are practically no longer available. The editors of CHRONICA BOTANICA *believe that much has been written in the past which is at least as valuable as quite a little of what is written today, especially in the borderlands between the humanities and biology. One could hardly ask for a stronger support for this view than presented by this stimulating collection of Stevensiana.*

F. V.

CONTENTS

— I —

RADICALISM AND RESEARCH IN AMERICA*

UNDOUBTEDLY the most radical document ever adopted by an American national assembly was the Declaration of Independence. The active members of the committee appointed to draft this instrument were FRANKLIN, ADAMS, and JEFFERSON, each of whom made a distinct contribution to the advancement of scientific foundations in America.

FRANKLIN's fame as a scientist, as a diplomat, and as leader of the radical faction in our constitutional convention makes comment on these points unnecessary. Of special interest here is his activity in founding our first academy of science. As early as 1743 BENJAMIN FRANKLIN issued his circular entitled "A proposal for promoting useful knowledge among the British plantations in America," in which he urged the establishment of a society to be called "The American Philosophical Society." From this Society and another organized in 1766, of which FRANKLIN was first president, grew in 1769 The American Philosophical Society of today. Of this society FRANKLIN was president from its organization until his death and Dr. BENJAMIN RUSH, one of the signers of the Declaration of Independence, was one of the secretaries.

To JOHN ADAMS, who in 1776 seconded the famous resolution of RICHARD HENRY LEE that "these colonies are, and of right ought to be free and independent states" and bore the foremost place in the debate on the adoption of the Declaration of Independence, our second Academy of Science owes its origin. Another of the original members of the American Academy of Arts and Sciences was LEVI LINCOLN, Attorney-General of the United States under JEFFERSON.

The activities of the author of the Declaration of Independence in behalf of science and education were varied and important.

In 1782 appeared JEFFERSON's "Notes on the state of Virginia," the first comprehensive treatise on the natural history and resources of one of the states, and the precursor of the numerous state surveys since issued. When in 1797 JEFFERSON came to Philadelphia to be inaugurated vice-president he brought with him a collection of the fossilized bones of some large quadruped and the manuscript of a memoir upon them, which he read before the American Philosophical Society.

JEFFERSON's presidency was probably the "most memorable in the his-

* First published in *Science* 52: 25-30 (1920).

tory of American science." Not only was the president actively engaged in paleontological research, using one of the unfinished rooms of the White House for the storage and display of some 300 specimens of fossil bones from the famous Big Bear Lick, but his administration was marked by the inception of the system of scientific surveys of the public domain and the organization of the Coast Survey. JEFFERSON's part in originating and supporting the LEWIS and CLARKE expedition is well known. That in JEFFERSON's mind at least, political radicalism and interest in higher education were clearly joined may be judged from the epitaph he himself prepared:

Here was buried THOMAS JEFFERSON, author of the Declaration of American Independence, of the statute of Virginia for religious freedom and father of the University of Virginia.

If this subject were pursued into the field of state and local history much relevant data could be presented. MERRILL[1] traces "the beginning of the work which resulted in the establishment of the State survey" in New York to a course of lectures on natural history delivered by AMOS EATON before the State Legislature in Albany during April, 1818, on the invitation of GOVERNOR DEWITT CLINTON. CLINTON, while best known historically for his work in behalf of the Erie Canal, was active in securing the abolition of slavery in New York state and in perfecting a system of free public schools and was the author of a series of letters signed "A Countryman" in reply to the "Federalist."

EDWARD HITCHCOCK's Survey of Massachusetts (1830-1833), which MERRILL refers to[2] as marking "an epoch in American geological work, since it brought to a successful conclusion the first survey of an entire state at public expense," was also a result of the interest of a radical governor, LEVI LINCOLN, (son of the LINCOLN mentioned above) who recommended the survey and Professor HITCHCOCK's appointment. Governor LINCOLN is known in the history of his state as the first governor to exercise the veto power, and as the leader of the minority in the Massachusetts State Legislature who protested against the Hartford convention of 1814.

Nor was the fostering of science and education wholly the concern of individual radicals at this period. For hardly had the Democratic majority in Maine effected the separation of the state from Federalist Massachusetts (1820) than the State Legislature made an annual grant of $1,000 to aid in maintaining an institution which was to give mechanics and farmers "such scientific education as would enable them to become skilled in their professions." This institution was incorporated as the Gardiner Lyceum and opened January 1, 1823.

The greatest radical movement after the Revolution was that which resulted in the abolition of slavery. Of those whose names have already appeared in this sketch, JEFFERSON and CLINTON were conspicuous advocates of abolition. The first prominent opponent in Congress of the extension of slavery was probably JOHN QUINCY ADAMS. One is not surprised to learn that this sturdy individualist who changed his political affiliations at will and maintained an influential position in Congress for many years, independent of party and who refused to be silenced by the "gag rule" of

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[1] MERRILL, GEORGE P., "Contributions to the History of American Geology," Rpt. U. S. Nat. Mus., 1903-04, p. 213, 1906.

[2] MERRILL, GEORGE P., *op. cit.*, p. 307.

1837 was deeply interested in science and its advancement. As outlined by GOODE[3] he revived Washington's National University project, worked for a national astronomical observatory, was actively interested in the foundation of the Smithsonian Institution and considered his most important achievement to be the Report on Weights and Measures prepared for Congress in 1818. Of this he was justly proud for it was a very admirable piece of scientific work. He found the presidency of the American Academy of Arts and Sciences so congenial to his tastes and sympathies that he did not hesitate to say that he prized it more highly than the chief magistracy of the nation.

The movement against slavery resulted in the election in 1854 of a majority in the House of Representatives of men pledged to oppose the extension of slavery. Among the members of the Republican majority which gained control of the House in 1855 was JUSTIN S. MORRILL, who in December, 1857, introduced a bill "donating public lands to the several States and Territories which may provide colleges for the benefit of agriculture and mechanic arts." This bill, though finally passed by Congress, was vetoed by the reactionary BUCHANAN. A similar bill, however, introduced by Mr. MORRILL in December 16, 1861, was passed by both Houses and approved by ABRAHAM LINCOLN, July 2, 1862. The chief significance of the Morrill Act for research lies in its relation to the subsequent and closely connected development of experiment stations first regularly organized under the Hatch Act, approved by President CLEVELAND, March 2, 1887.

Although the chief energies of LINCOLN's administration were turned toward the prosecution of our Civil War, Congress passed a bill establishing a Department of Agriculture, an act which became law by approval of President LINCOLN on the 15 of May, 1862.

Even during the trying days of reconstruction members of the first Republican Congress did not neglect scientific investigation and "in the spring of 1867 HAYDEN [F. V.] acting under the direction of the General Land Office, and with an appropriation from Congress amounting to $5,000, began his work as U. S. geologist in Nebraska, and in so doing laid the foundation for the U. S. Geological Survey."

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When first published, the title of this article, perhaps more than its contents, disturbed some readers whose patriotism exceeded their knowledge of the English language. The words "radical" and "radicalism" were used with what I still believe to be their correct connotation. A radical reform is one which is fundamental. A radical politician is one who advocates fundamental reforms.

This sketch, written in 1920, actually includes no item later than 1887. The events of the last sixty years in the United States have done nothing to weaken the main thesis. This was merely that administrations which in their day stood well to the left, have often been those in which science received liberal governmental aid and encouragement. Detailed review of this period would be tedious. It may not be out of place, however, to mention a few items that come readily to mind. The record of the LA FOLLETTES in Wisconsin is well known. The Adams Act, which like the earlier Hatch Act grants federal aid for research to state experiment stations, came during the administration of THEODORE ROOSEVELT. That of F. D. ROOSEVELT saw the Bankhead-Jones Act and the establishment of the "Regional Laboratories." The National Research Council was established by WOODROW WILSON and the movement which almost succeeded in creating a National Science Foundation began under F. D. ROOSEVELT.

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[3] GOODE, GEORGE BROWN, "The Origin of the National Scientific and Educational Institutions of the United States," Rpt. U. S. Nat. Mus., 1896-97, pp. 302-311, 1901.

— II —

THE OBLIGATION OF THE INVESTIGATOR TO THE LIBRARY*

THE dependence of the present-day investigator upon institutional libraries is almost absolute. Necessarily so, as only a very exceptional person can own, or provide room for, a library complete enough to cover the range of his professional interests. Even if he owned the books he could not care for them and do anything else. Except in his own special field, no investigator will attempt to compete with the skilled bibliographers of our better libraries, and even in his own field he is apt to appear at a disadvantage. One of the most careful workers of my acquaintance recently located, after much search, the title of a somewhat obscure work on stomata, only to find, shortly after, that the book was plainly catalogued under the heading STOMATA in the library of the institution in which he was at work.

The work of the librarian is important to the investigator not only in making the results of previous researches available now, but in the attempt to insure present results being available in the future. If the results of the investigations of today are anywhere available to succeeding generations it will be in the larger libraries where the publications containing them are being carefully collected and catalogued. We have heard much recently about cooperation among investigators, its desirability, its difficulty, and its disadvantages, and the means by which its undesirable features may be avoided and its disadvantages and difficulties lessened. Might not brief consideration well be given to cooperation between the investigator and the most important of his colaborers, a cooperation which can have neither difficulties nor disadvantages?

Those of us who are much in the field, perhaps, appreciate more keenly than those who are always in touch with their homes the special advantages of the public library. In these days of closed bars and crowded hotels the one place where the stranger is sure of a welcome is the public library. And, speaking seriously, the importance and influence in small communities of libraries as well stocked and well conducted as those of Poughkeepsie, New York, and Riverside, California, for example are hard to estimate. Now that Mr. CARNEGIE has provided these institutions all over the country with

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*First published in *Science* 52:223-225 (1920).

suitable buildings, in his commendable effort to die poor, why should not the investigator, who must die poor anyway, look to their contents?

The smaller public libraries need help especially in this particular. The almost overwhelming demand on these libraries for fiction, especially recent fiction, should not be permitted to exclude scientific material from their shelves. If the results of our labors, or the methods, or even the activities themselves, are to be made known to the reading public, as much of our literature as possible must be made available in public libraries. Every public library should have at least *Science* and the *Scientific Monthly.* If you find a library that lacks them, urge the authorities to subscribe, and if they lack the funds, give them your own set.

The investigator has, moreover, an obligation to the college library, the library of the college from which he graduated perhaps, or the one nearest his home. Other alumni will care for other interests, the pious for the erection of a new chapel, the more worldly minded for the gymnasium, but the library is too often left to shift for itself, and provided with insufficient funds. This applies particularly to the smaller colleges of course, but it is indeed a rare university library to which the average investigator can not add some volume in the course of ten years' work, and that volume will on the whole be much more useful and safer in a good library than in a private study or laboratory.

From the standpoint of self-interest as well as of common honesty, however, the first duty of the investigator is to the reference libraries, whether general libraries like the John Crerar Library and those of our leading universities, or libraries covering special fields such as the Lloyd Library or those connected with our large botanic gardens. If an investigator accepts the hospitality and uses the facilities of the Library of the Marine Biological Laboratory at Woods Hole, or that of Stanford University, and one is made quite at home in both without introduction, it seems no more than fair that these libraries be supplied in return with as complete a set as possible of his own publications if they lie within the field of interest of the library. I am reliably informed that this practice is by no means general. Comparatively few of the investigators of my acquaintance take the trouble to send reprints of their publications even to the Library of Congress.

That these papers are usually published in standard periodicals, of which complete sets are supposedly available in these libraries does not cover the case. The United States at least is afflicted with several scientific periodicals of avowedly general nature, and some of the special journals have a none too stable editorial policy. Some of these special journals moreover still further complicate bibliographical work by permitting the publication of abstracts of work which at some time may be judged worthy of adequate publication, thus cluttering their indices beyond the point of convenience if not utility.

If then our libraries, even our special libraries, are to approximate completeness in their indices of current published scientific material they should have the assistance of the investigators themselves, at least to the extent of supplying them with such articles as are reprinted for private circulation. It is an almost universal custom for investigators to distribute reprints of their own papers among their colleagues. To add to these private mailing

lists the names of the fifty leading libraries of this and other countries would mean some trouble and some slight expense. The time and cost thus involved would however be a very small fraction indeed of that expended in the prosecution and publication of the work. The insurance thus purchased that the papers would be cared for and made more available to this and succeeding generations would be well worth the investment.

—III—

THE BOTANY OF THE NEW ENGLAND POETS*

IN THE paper published under the above title in 1921, attention was confined to the poets of that little group usually known as the Cambridge School, namely LONGFELLOW, WHITTIER, HOLMES, and LOWELL. Some revision of an article a quarter century old seems permissible, particularly if this results in condensation.

This revision does not alter the main thesis which was that abundant evidence of accurate knowledge of plants is found in the poems of HOLMES and LOWELL. Their use of such material is, of course, quite different. In the writings of LONGFELLOW and WHITTIER, on the other hand, one finds far fewer references to plants, and less indication of interest in them. The temptation is very great to relate these facts to the conditions surrounding the boyhood of the several poets. It is in the poetry of the two who grew up in the then straggling inland village of Cambridge, under circumstances of abundant leisure, that we find the greatest knowledge of, and appreciation of, plants.

In LONGFELLOW's poems there are to be found, of course, fine lines which express realization of the beauty of field and forest, and there is evidence of some botanical knowledge. No careful reader of his poetry will, however, maintain that LONGFELLOW appreciated plants or plant life as he did the ocean, or even inland bodies of water. Some explanation of this may perhaps be found in the surroundings of his youth. Born within sight of the sea and his boyhood spent in a seaport town, it is natural that the ocean should have left an impression on his mind unequaled by other natural objects. Indeed in "My Lost Youth" the first five stanzas deal chiefly with the sea and the harbor including:

I remember the black wharves and the slips,
And the sea-tides tossing free;
And the Spanish sailors with bearded lips,
And the beauty and mystery of the ships,
And the magic of the sea.

Whereas the only references to things botanical are to the trees that line the streets, and to Deering's Wood, a grove in the outskirts of Portland.

WHITTIER; most of whose early life was spent on a farm, and in hard work on that farm, knew the names of a good many plants, but even when

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*First published in *The Scientific Monthly* 12: 137-149, 1921 (somewhat condensed).

he mentions them it is often in their relation to human life or as symbolic of human activity. On receiving a sprig of heather in blossom, he writes a poem for "Burns" and in writing of "The Sycamores" his thoughts are very little with the trees but chiefly of the Irishman, HUGH TALLANT, who planted them. The trailing arbutus moves him to two poems; but one "The Mayflowers," deals chiefly with the Pilgrims, and a shorter one "The Trailing Arbutus" ends thus:

As pausing, o'er the lonely flower I bent,
I thought of lives thus lowly, clogged and pent,
Which yet find room,
Through care and cumber, coldness and decay,
To lend a sweetness to the ungenial day,
And make the sad earth happier for their bloom.

As a boy WHITTIER apparently showed no special interest in plants. In fact, it would seem that such biological inclinations as he possessed were zoological rather than botanical. If one examines "The Barefoot Boy's" stock of nature lore

Knowledge never learned of schools,
Of the wild bee's morning chase,
Of the wild-flower's time and place,
Flight of fowl and habitude
Of the tenants of the wood;
How the tortoise bears his shell,
How the woodchuck digs his cell,
And the ground-mole sinks his well;
How the robin feeds her young,
How the oriole's nest is hung;
Where the whitest lilies blow,
Where the freshest berries grow,
Where the groundnut trails its vine,
Where the wood-grape's clusters shine;
Of the black wasp's cunning way,
Mason of his walls of clay,
And the architectural plans
Of gray hornet artisans!—

he will come to the conclusion that this particular boy was much more interested in animals than in plants. For against a very creditable list of items of information regarding quadrupeds, birds, and insects are balanced only two generalizations about flowers, and three plants regarded as sources of food.

In "The Pumpkin" he exclaims with many another New Englander:

What moistens the lip and what brightens the eye?
What calls back the past, like the rich Pumpkin pie?

HOLMES' medical training gave him a knowledge of drug plants, which he uses to good advantage in a professional poem "Rip Van Winkle, M.D." HOLMES seems to have had, moreover, a special interest in and affection for trees. References to them occur again and again in his poems. That this interest dates from his boyhood is evidenced by a poem "The Meeting of the Dryads" written after a general pruning of the trees around Harvard College, some time before he was sixteen. With real feeling the spirit of one of the mutilated trees speaks:

Go on, Fair Science; soon to thee
Shall Nature yield her idle boast;
Her vulgar fingers formed a tree,
But thou hast trained it to a post.

and the poem concludes with the following inclusive curse upon the "tree surgeon" of Harvard:

A curse upon the wretch who dared
To crop us with his felon saw!
May every fruit his lip shall taste
Lie like a bullet in his maw.

. . . .

May nightshade cluster round his path,
And thistles shoot, and brambles cling;
May blistering ivy scorch his veins,
And dogwood burn, and nettles sting.

On him may never shadow fall,
When fever racks his throbbing brow,
And his last shilling buy a rope
To hang him on my highest bough!

HOLMES most frequently uses botanical information in figures, and his botanical figures are usually apt.

With a vigor equaled only by the preacher's "of making many books there is no end; and much study is a weariness of the flesh" he sums up the passion for publication, chiefly in botanical figures, in Cacoethes Scribendi:

If all the trees in all the woods were men;
And each and every blade of grass a pen;
If every leaf on every shrub and tree
Turned to a sheet of foolscap; every sea
Were changed to ink, and all earth's living tribes
Had nothing else to do but act as scribes,
And for ten thousand ages, day and night,
The human race should write, and write, and write,
Till all the pens and paper were used up,
And the huge inkstand was an empty cup,
Still would the scribblers clustered round its brink
Call for more pens, more paper, and more ink.

Perhaps his best figure, certainly his best known, is a botanical one, "The Last Leaf." It is a curious commentary on the failure of many people to observe the most common natural phenomena that in the later editions of his poems, HOLMES felt obliged to insert in the introduction to the poem bearing the above title an explanation of a relation which should have been obvious to every one who had seen oak trees in New England in early spring. His own fondness for so apt a figure is shown by its repeated use. In 1831 (The Last Leaf) he wrote:

And if I should live to be
The last leaf upon the tree
In the spring,
Let them smile, as I do now,
At the old forsaken bough
Where I cling.

Thirty years later he uses the same figure (The Old Player 1861):

Yet there he stood,—the man of other days,
In the clear present's full, unsparing blaze,
As on the oak a faded leaf that clings
While a new April spreads its burnished wings.

And in 1889 in acknowledging the gift of a silver loving cup, he wrote (To The Eleven Ladies):

"For whom this gift?" For one who all too long
Clings to his bough among the groves of song;
Autumn's last leaf, that spreads its faded wing
To greet a second spring.

LOWELL was deeply interested in the turbulent political events of his time and The Biglow Papers are the soul of his published work. However, his poems bear evidence of deep interest in and accurate knowledge of natural objects, particularly plants. He does not seem to overstate the case when in To George William Curtis he tells of his interest in out of door things:

Dear were my walks, too, gathering fragrant store
Of Mother Nature's simple-minded lore:
I learned all weather-signs of day or night;
No bird but I could name him by his flight,
No distant tree but by his shape was known,
Or, near at hand, by leaf or bark alone.
This learning won by loving looks I hived
As sweeter lore than all from books derived.
I know the charm of hillside, field and wood,
Of lake and stream, and the sky's downy brood,
Of roads sequestered rimmed with sallow sod,
But friends with hardhack, aster, golden-rod,[4]
Or succory keeping summer long its trust
Of heaven-blue fleckless from the eddying dust:
These were my earliest friends, and latest too,
Still unestranged, whatever fate may do.

Whatever LOWELL's knowledge of botany he certainly understood and appreciated scientists and quite properly places Jonah among them. (The Biglow Papers)

Men in general may be divided into the inquisitive and the communicative. To the first class belong Peeping Toms, eaves-droppers, navel-contemplating Brahmins, metaphysicians, travellers, Empedocleses, spies, the various societies for promoting Rhinothism, Columbuses, Yankees, discoverers, and men of science, who present themselves to the mind as so many marks of interrogation wandering up and down the world, or sitting in studies and laboratories. . . .

To one or another of these species every human being may safely be referred. I think it beyond a peradventure that Jonah prosecuted some inquiries into the digestive apparatus of whales, and that Noah sealed up a letter in an empty bottle, that news in regard to him might not be wanting in case of the worst. They had else been super or subter human.

Horticulture:— It is difficult to determine just what should be considered as significant horticultural information.

Here should certainly be included, however, an observation made by LOWELL in a note introductory to one of the Biglow Papers

The two faculties of speech and of speech-making are wholly diverse in their natures. By the first we make ourselves intelligible, by the last unintelligible, to our fellows. It has not seldom occurred to me (noting how in our national legislature everything runs to talk, as lettuces, if the season or the soil be unpropitious, shoot up lankly to seed, instead of forming handsome heads) that Babel was the first Congress, the earliest mill erected for the manufacture of gabble.

[4] For the guidance of those unfamilar with the common names there are included in several places lists of the plants mentioned with their Latin names as given in GRAY's Manual, Seventh Edition. Hardhack, *Spiraea tomentosa* L.; Golden-rod, *Solidago sp.;* Succory, *Cichorium Intybus L.*

The horticultural significance of this quotation lies of course in the parenthetical observation that under conditions unfavorable for vegetative growth, lettuce like many other vegetables runs to seed.

In these days when grades and standards for packing fruit, and the accurate labeling of foods and drugs are an important part of the activity of a Department of Agriculture it may be interesting to note some of the things HOLMES considered "essential to a Millennium" (Latter-Day Warnings)

When legislators keep the law,
When banks dispense with bolts and locks—
When berries—whortle, rasp, and straw—
Grow bigger downwards through the box,

. . . .

When preachers tell us all they think,
And party leaders all they mean,
When what we pay for, that we drink,
From real grape and coffee-bean,

. . . .

Till then let Cumming blaze away,
And Miller's saints blow up the globe;
But when you see that blessed day,
Then order your ascension robe!

We are indebted to WHITTIER's interest in all that pertained to advocates of the abolition of negro slavery, for a poetic record of seed and plant introduction carried on previous to 1700 by FRANCIS DANIEL PASTORIUS, one of the earliest German immigrants to Pennsylvania. According to WHITTIER a correspondence and exchange of plants was carried on between PASTORIUS and his friends and teachers in Germany (The Pennsylvania Pilgrim):[5]

And thus the Old and New World reached their hands
Across the water, and the friendly lands
Talked with each other from their severed strands.

Pastorius answered all: while seed and root
Sent from his new home grew to flower and fruit
Along the Rhine and at the Spessart's foot.

And, in return, the flowers his boyhood knew
Smiled at his door, the same in form and hue,
And on his vines the Rhenish clusters grew.

Mycology and Phytopathology:— In view of the scant consideration given fungi by New England botanists before FARLOW, it may be worth mentioning that at least two of the New England poets knew that fungi existed. HOLMES mentions fungi frequently. One of his early poems is entitled The Toadstool, and EMERSON refers to Nature to:

Where the fungus broad and red
Lifts its head,
Like poisoned loaf of elfin bread.

Moreover, from two chance references I am convinced that HOLMES knew, what some botanists had yet to learn, that a lichen is really a fungus. In

[5] JELLETT, EDWIN C., Germantown Gardens and Gardeners, 1914. On pages 5-16 of this work is given an account of FRANCIS DANIEL PASTORIUS, the founder of Germantown, which indicates that WHITTIER's account is substantially correct.

Astraea he contrasts the "arctic fungus" and the desert palm: and in "A modest request" he refers to:

—the rock where nature flings
Her arctic lichen, last of living things;

Mention of plant diseases is confined chiefly to generalized references to mold, mildew and blight, but in the Biglow Papers there is evidence that LOWELL knew Peach Yellows and some of its symptoms.

Take them editors thet's crowin'
Like a cockerel three months old,—
Don't ketch any on 'em goin',
Tho they be so blasted bold;
Aint they a prime lot o' fellers?
'Fore they think on't guess they'l sprout
(Like a peach thet's got the yellers)
With the meanness bustin' out.

Ecology:— In these days when ecology has a society, a special journal, and a special language it would be a presumptuous botanist indeed, who, not one of the elect, should attempt to decide what is ecology and what is not, and it would be even more presumptuous to suppose that in poets of the last generation one would find today's ecology. But in the old-fashioned sense of plant relations, the relations of plants to each other and to their environment we find, in LOWELL at least, evidence of real comprehension. Any one who has carefully watched the almost unbelievably rapid development of vegetation during the spring of northern New England, and contrasted it with the abject manner in which spring approaches in southern California will appreciate the feeling and the keen observation evidenced in LOWELL's description of spring in the Biglow Papers. Beginning on May Day:

I, country-born an' bred, know where to find
Some blooms thet make the season suit the mind,
An' seem to metch the doubtin' bluebird's notes,—
Half-ventrin' liverworts[6] in furry coats,
Bloodroots, whose rolled-up leaves ef you oncurl,
Each on 'em's cradle to a baby-pearl,—
But these are jes' Spring's pickets; sure ez sin,
The rebble frosts 'll try to drive 'em in;
For half our May's so awfully like May n't,
't would rile a Shaker or an evrige saint;
Though I own up I like our back'ard springs
Thet kind o' haggle with their greens an' things,
An' when you 'most give up, 'uthout more words
Toss the fields full o' blossoms, leaves an' birds:

. . . .

'fore long the trees begin to show belief,
The maple crimsons to a coral-reef,
Then saffern swarms swing off from all the willers
So plump they look like yaller caterpillars,
Then gray hossches' nuts leetle hands unfold
Softer'n a baby's be at three days old:

. . . .

Then seems to come a hitch,—things lag behind,
Till some fine mornin' Spring makes up her mind,
An' ez, when snow-swelled rivers cresh their dams
Heaped-up with ice thet dovetails in an' jams,

A leak comes spirtin' through some pin-hole cleft,
Grows stronger, fercer, tears out right an' left,
Then all the waters bow themselves an' come,
Suddin, in one gret slope o' shedderin' foam,
Jes' so our Spring gits everythin' in tune
An' gives one leap from Aperl into June:
Then all comes crowdin' in; afore you think,
Young oak-leaves mist the side-hill woods with pink;
The catbird in the laylock-bush is loud;
The orchards turn to heaps o' rosy cloud;
Red-cedars blossom tu, though few folks know it,
An' look all dipt in sunshine like a poet;

Not only is the succession of spring flowers correctly observed, and appropriate emphasis given to the rapidity of the development of the vegetation during the latter part of May, but the above quoted poem is the only record the writer has found of the fact that in New England at least the red-cedar blossoms at the same time as the cultivated apple.

HOLMES expressed the idea of the rapid climatic change at this season with equal vigor in an early poem entitled "The Hot Season":

The folks, that on the first of May
Wore winter coats and hose,
Began to say, the first of June,
"Good Lord! how hot it grows!"

So far as vegetation is concerned, however, HOLMES' idea of spring includes, as becomes a townsman, chiefly cultivated plants. In three poems (Poetry, Spring, and Spring Has Come) in which the season is described he mentions the snowdrop, the hyacinth, tulip, crocus, iris, narcissus, and peony; but among wild flowers only the violet. Not less accurate, nor less vivid than his description of spring is LOWELL's record of autumn foliage in An Indian Summer Reverie.

Few phenomena more often interest the botanist in rural New York and New England than the persistence of certain plants about the sites of abandoned homesteads. HOLMES records the plants he observed about one such in The Exile's Secret.

Who sees unmoved, a ruin at his feet,
The lowliest home where human hearts have beat?
Its hearthstone, shaded with the bistre stain
A century's showery torrents wash in vain;
Its starving orchard, where the thistle blows
And mossy trunks still mark the broken rows;
Its chimney-loving poplar, oftenest seen
Next an old roof, or where a roof has been;
Its knot-grass, plantain,—all the social weeds,
Man's mute companions, following where he leads;

. . . .

Its woodbine, creeping where it used to climb;
Its roses, breathing of the olden time;

Whether the original inhabitant of this "Island Ruin" was so unconventional as not to have had a lilac, or the lilac was unable to persist in so

[6] Liverwort, *Hepatica triloba* Chaix; Bloodroot, *Sanguinaria canadensis* L.; Maple, Acer (probably *rubrum* L.); Willers (willow), *Salix sp.;* Hossches' nuts (Horse-chestnut), *Aesculus Hippocastanum* L.; Oak, *Quercus sp.;* Laylock (lilac), *Syringa vulgaris* L.; Red-cedar, *Juniperus virginiana* L.

exposed a place cannot, of course, be determined. But it is hardly to be supposed that HOLMES could have missed what is usually the most conspicuous of the relics. WHITTIER reports it in a similar situation (The Homestead):

A lilac spray, once blossom clad,
Sways bare before the empty rooms;
Beside the roofless porch a sad
Pathetic red rose blooms.

Wood Technology:— HOLMES is probably supreme in this field although the landlord in LONGFELLOW's Tales of A Wayside Inn knows the fuel value of Oak, Maple and Apple, and in Hiawatha the canoe is built from appropriate and serviceable materials. In Poetry HOLMES shows that he knows some of the penalties for using green wood for construction purposes, and it is no poetic license, but accurate observation of natural phenomena that records the sprouting from the unhewn timbers on the shady, and moister, side.

Scarce steal the winds, that sweep his woodland tracts,
The larch's[7] *perfume from the settler's axe,*
Ere, like a vision of the morning air,
His slight framed steeple marks the house of prayer;
Its planks all reeking and its paint undried,
Its rafters sprouting on the shady side,

Likewise the Deacon in building his now famous One-hoss shay uses, according to HOLMES' record, the best of materials and judgment. He

inquired of the village folk[8]
Where he could find the strongest oak,
That couldn't be split nor bent nor broke,—
That was for spokes and floor and sills;
He sent for lancewood to make the thills;
The crossbars were ash, from the straightest trees,
The panels of white-wood, that cuts like cheese,
But lasts like iron for things like these;
The hubs of logs from the "Settler's ellum,"—
Last of its timber,—they couldn't sell 'em,
Never an axe had seen their chips,
And the wedges flew from between their lips,
Their blunt ends frizzled like celery-tips;

Careful review of the woods used, in the light of the information now available, leads one to believe that when combined with "Steel of the finest, bright and blue" they would go far to make good the Deacon's vow that

He would build one shay to beat the taown
'N' the keounty 'n' all the kentry raoun';
It should be so built that it could n' break daown;
"Fur," said the Deacon, "'t's mighty plain
That the weakes' place mus' stand the strain;
'N' the way t' fix it, uz I maintain,
Is only jest
T' make that place uz strong uz the rest."

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[7] Larch, *Larix laricina* (Du Roi) Koch.

[8] The only mention of the botanical significance of this poem which the writer has ever seen in print is the "Young People's Arbor" conducted by JAMES LAWLER, Canadian Forestry Journal 11: 201-202 (1915).

Oak undoubtedly for this use, *Quercus alba* L.

Lancewood, The West Indian *Oxandra virgata* ("He sent for lancewood.")

Ash, *Fraxinus americana* L.; White-wood, *Liriodendron Tulipifera* L.;

Elm, *Ulmus americana* L.

Though only by the most extreme care in construction, and a timber inspection even more rigid than that used in the construction of airplanes could one guarantee a result like that reported by HOLMES, for

Little of all we value here
Wakes on the morn of its hundredth year
Without both feeling and looking queer, ,
In fact there is nothing that keeps its youth,
So far as I know, but a tree and truth.

It is, then not surprising that at this age,

There are traces of age in the one-hoss shay,
A general flavor of mild decay,
But nothing local, as one may say.

or that the dramatic conclusion of the experiment occurs on this **day.**

All at once the horse stood still,
Close by the meet'n'-house on the hill,
First a shiver, and then a thrill,
Then something decidedly like a spill,—
And the parson was sitting upon a rock,
At half past nine by the meet'n'-house clock,—
Just the hour of the Earthquake shock!
What do you think the parson found,
When he got up and stared around?
The poor old chaise in a heap or mound,
As if it had been to the mill and ground!
You see, of course, if you're not a dunce,
How it went to pieces all at once,—
All at once, and nothing first,—
Just as bubbles do when they burst.

— IV —

BOTANICAL FIGURES IN BIBLICAL PROPHECY*

WHATEVER the message of the Hebrew prophets, their language was vigorous. One element which made their speech telling was the use of figures of wide appeal, and almost universally understood. Nor were their figures significant only at the time they were spoken. In very large part they are as readily understood today as several thousand years ago. Prominent among these enduring figures are those here designated as botanical, because they refer to plants.

The usefulness of a classical allusion, or even a Biblical reference, is definitely limited. Such a figure can appeal only to those familiar with the literature, or to whom the figure has been explained. A botanical figure, on the other hand, appeals wherever there are people and plants. That the plant names used in the English Bible are in some cases certainly not those referred to in the original, in no way affects the truth of this assertion. It is indeed additional proof of the permanence of these botanical figures that they have survived both time and translation. Botanical figures are of course not unique in this respect. In the same class are all references to natural objects or phenomena. For example,

> O Judah, what shall I do unto thee? for your goodness is as a morning cloud, and as the dew that goeth early away. (HOSEA.)[1]
>
> For they sow the wind, and they shall reap the whirlwind. (HOSEA.)

Nor are botanical figures confined to or especially developed in the prophetic books. The Song of Songs contains no less than sixteen, and the First Psalm consists essentially of two contrasted botanical figures. The prophetic books are selected for discussion because they form a fairly homogeneous group, and it is obviously impracticable to consider the Scriptures entire. Even here no attempt will be made at complete citation. The aim will be rather to indicate the range of ideas which botanical figures are used to amplify.

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* First published in *The Methodist Review* 104: 419-425 (1921).

[1] As will be at once evident, the text used is that of the Modern Reader's Bible, edited by RICHARD G. MOULTON. For permission to quote from this copyrighted text the writer wishes to acknowledge his obligation to The Macmillan Company. No indication is made of chapter and verse, for the quotations will be easily verified by those interested and references would needlessly cumber the text.

Perhaps no idea is more naturally expressed in botanical figure than that of growth and development. This may be either the development of an idea, or the actual growth of a nation, with the closely associated idea of reproduction.

For as the earth bringeth forth her bud, and as the garden causeth the things that are sown in it to spring forth; so the Lord God will cause righteousness and praise to spring forth before all nations. (ISAIAH.)

In days to come shall JACOB take root;
Israel shall blossom and bud:
And they shall fill the face of the world with fruit. (ISAIAH.)

I caused thee to multiply as the bud of the field. (EZEKIEL.)

I will pour my spirit upon thy seed, and my blessing upon thine offspring: and they shall spring up among the grass, as willows by the watercourses. (ISAIAH.)

The significance of the last quotation will be best appreciated by those who recall the readiness with which the numerous dehiscent branches of the willow take root in moist soil, its abundant seed production, and the extraordinarily rapid and abundant germination of the seed under favorable moisture conditions, "by the watercourses."

An idea which finds frequent expression in the prophetic books, especially ISAIAH, is that of the small and carefully chosen "remnant" which shall survive the universal judgment and be reestablished, "again take root downward and bear fruit upward" (ISAIAH) in a golden age. This remnant is often described in botanical figures.

For thus shall it be in the midst of the earth among the peoples, as the shaking of an olive tree, as the grape gleanings when the vintage is done. (ISAIAH.)

Yet there shall be left therein gleanings, as the shaking of an olive tree, two or three berries in the top of the uppermost bough, four or five in the outmost branches of a fruitful tree, saith the Lord, the God of Israel. (ISAIAH.)

And the remnant of the trees of his forest shall be few, that a child may write them. (ISAIAH.)

And it shall come to pass in that day, that the Lord shall beat out his corn, from the flood of the River unto the brook of Egypt, and ye shall be gathered, one by one, O ye children of Israel. (ISAIAH.)

ISAIAH finds botanical figures equally applicable to express desolation and its antithesis. And material prosperity finds its counterpart in fruitful vegetation.

For ye shall be as an oak whose leaf fadeth, and as a garden that hath no water. (ISAIAH.)

The wilderness and the solitary place shall be glad; and the desert shall rejoice, and blossom as the rose. (ISAIAH.)

Wherefore doth the way of the wicked prosper? wherefore are all they at ease that deal treacherously? Thou hast planted them, yea, they have taken root; they grow, yea, they bring forth fruit. (JEREMIAH.)

Serenity and joy among the mental states, as well as physical strength and weakness are expressed botanically.

And their soul shall be as a watered garden; and they shall not sorrow any more at all. (JEREMIAH.)

And ye shall see it, and your heart shall rejoice, and your bones shall flourish like the tender grass. (ISAIAH.)

Yet destroyed I the Amorite before them, whose height was like the height of the cedars, and he was as strong as the oaks; yet I destroyed his fruit from above, and his roots from beneath. (AMOS.)

Therefore their inhabitants were of small power, they were dismayed and confounded; they were as the grass of the field, and as the green herb, as the grass on the housetops, and as a field of corn before it be grown up. (ISAIAH.)

The figure of a "bruised reed" to express unreliability has, like many other Biblical figures, entered into our daily speech.

Behold, thou trusteth upon the staff of this bruised reed, even unto Egypt; whereon if a man lean, it will go into his hand, and pierce it; so is Pharaoh king of Egypt to all that trust on him. (ISAIAH.)

Equally vigorous, though less well known, is the figure used by NAHUM to express the weakness of fortification.

All thy fortresses shall be like fig trees with the firstripe figs:
If they be shaken,
They fall into the mouth of the eater. (NAHUM.)

The significance of this figure lies in the fact that the figs of the first crop (the first ripe) are often larger than those which mature later and would thus be more easily detached by shaking. That the first crop of a fig tree each season is, in some varieties, generally composed of larger fruit than the later crops is attested by such well known authorities as Dr. J. E. COIT of California and Dr. W. T. SWINGLE of Washington. These gentlemen are, however, unable to verify the observations of the prophet HOSEA that the crop of the first year is usually superior in quality to that of later years.

I found Israel like grapes in the wilderness; I saw your fathers as the firstripe in the fig trees at her first season. (HOSEA.)

Of course it is possible that the English translation does not exactly represent the original or that in the variety best known to this early writer such a condition may actually have been noticed. The first year's crop on some of our modern varieties of other fruits is often composed of notably large and beautiful specimens.

In discussing the essentials of a liberal education, advocates of the study of Greek and Latin maintain, or used to maintain, that they were necessary to a proper appreciation of English literature. Those who are now advocating the study of the English Bible as literature in our schools urge that the Hebrews no less than the Greeks were our literary ancestors; and that to train ourselves in the productions of one and not of the other means a distorted culture. After considering such figures as those just mentioned it may well be maintained that for a proper understanding of the Bible one must have a good knowledge of botany and the other natural sciences.

The degenerate and desperate condition of a people who hunted "every man his brother with a net," whose judges were ready for a reward, and among whom there was "none upright" is thus described by MICAH:

The best of them is a brier: the most upright is worse than a thorn hedge. (MICAH.)

And among the many figures used to express the utter detestation felt for a deposed tyrant in the fourteenth chapter of ISAIAH, which seemed so appropriate in the days which followed the eleventh of November, 1918, is that of a useless and presumably diseased pruning.

But thou art cast forth away from thy sepulchre,
Like an abominable branch. (ISAIAH.)

Complete destruction is vividly expressed in the means by which a weed or cultivated plant may be injured beyond recovery.

EPHRAIM is smitten, their root is dried up, they shall bear no fruit. (HOSEA.)
And I will kill thy root with famine,
And thy remnant shall be slain. (ISAIAH.)

Radical change and regeneration finds its counterpart when desirable and useful plants replace undesirable or harmful ones.

Instead of the thorn shall come up the fir tree,
And instead of the brier shall come up the myrtle tree. (ISAIAH.)

JEREMIAH twice makes use of the parallel between the fruit of a tree and the results of a man's work, best known probably from the New Testament "by their fruits ye shall know them."

I knew not that they had devised devices against me, saying, let us destroy the tree with the fruit thereof, and let us cut him off from the land of the living, that his name may be no more remembered. (JEREMIAH.)

They have sown wheat, and have reaped thorns; they have put themselves to pain and profit nothing: and ye shall be ashamed of your fruits, because of the fierce anger of the Lord. (JEREMIAH.)

In spite of its familiarity the figure of the sour grapes should not be passed over without quotation, nor should that of the rush.

The fathers have eaten sour grapes,
And the children's teeth are set on edge. (EZEKIEL.)
Is it to bow down his head as a rush? (ISAIAH.)

Even partial quotation, however, is impossible of the sustained, almost intricate, botanical figures of EZEKIEL as used in the Parable of the Vine, the Parable of the Eagles and the Cedar, The Fallen Cedar, and A Wail for a Broken Vine. The vine as the national emblem of Israel appears frequently in a figurative sense.

The contrasting ideas of the brevity of human life and of the enduring life of a nation or of the things of God are most forcefully called up by the contrast of the annual herbaceous plant or plant part, and the long life of a sound and thrifty tree.

For as the days of a tree shall be the days of my people, and my chosen shall long enjoy the work of their hands. (ISAIAH.)[2]

Therefore they shall be as the morning cloud, and as the dew that passeth early away, as the chaff that is driven with the whirlwind out of the threshing-floor, and as the smoke out of the chimney. (HOSEA.)

All flesh is grass,
And all the goodliness thereof is as the flower of the field:
The grass withereth,
The flower fadeth,
Because the breath of the Lord bloweth upon it:
Surely the people is grass!
The grass withereth,
The flower fadeth:
But the word of our God shall stand forever. (ISAIAH.)

In JEREMIAH both members of the antithesis are supported by botanical figures in a passage of great force and beauty.

Thus saith the Lord: Cursed is the man that trusteth in man, and maketh flesh his arm, and whose heart departeth from the Lord. For he shall be like the heath in the

[2] Compare HOLMES'

In fact, there's nothing that keeps its youth,
So far as I know, but a tree and truth.

desert, and shall not see when good cometh; but shall inhabit the parched places in the wilderness, a salt land and not inhabited.

Blessed is the man that trusteth in the Lord, and whose hope the Lord is. For he shall be as a tree planted by the waters, and that spreadeth out his roots by the river, and shall not fear when heat cometh, but his leaf shall be green; and shall not be careful in the year of drought, neither shall cease from yielding fruit. (Jeremiah.)

These are of course the thoughts and in part the very words of the first psalm, which in spite of its familiarity may well be quoted in conclusion:

THE TREE AND THE CHAFF

A Prefatory Psalm

Blessed is the man that walketh not in the counsel of the wicked,
Nor standeth in the way of sinners,
Nor sitteth in the seat of the scornful.
But his delight is in the law of the Lord;
And in his law doth he meditate day and night.

And he shall be like a Tree planted by the streams of water,
That bringeth forth its fruit in its season,
Whose leaf also doth not wither;
And whatsoever he doeth shall prosper.
The wicked are not so;
But are like the Chaff which the wind driveth away.

Therefore the wicked shall not stand in the judgment,
Nor sinners in the congregation of the righteous.
For the Lord knoweth the way of the righteous:
But the way of the wicked shall perish.

— V —

BREVITY AT BOTANICAL BANQUETS*

THIS summer's meeting at Ithaca almost doubled for the year 1926 the opportunities for botanical speech-making in the United States. It may then be not inappropriate or impertinent to discuss the length desirable in such addresses.

Reference is, of course, not here made to papers of a highly specialized character before the various sections of the different societies. These sections are so numerous and so small that no one feels any obligation to attend and each feels perfectly free to leave whenever he chooses. The speaker is then under no obligation to conserve the time of his audience, if any. His position is somewhat like that of an author of a book, who may suit himself or his publisher as to its length, since the weary reader may easily lay the book aside or throw it into the fire.

The obligation for brevity in personal conversation is slight also, for the bored listener can usually escape, either by tact or violence. Whoever addresses an assembly, however, especially one to which people come partly for social reasons and where the auditors feel under compulsion to hear the speaker through, is under a definite obligation to be brief. Not every speaker, of course, can attain the brevity or awaken the enthusiasm aroused by Dr. BRITTON vicariously in his presidential address at Toronto, but a short speech may be, indeed often is, a good speech.

Brevity in speech, even serious speech, is possible. LINCOLN's Gettysburg address contains but 266 words. PAUL's speech on Mars Hill contains, in the English translation, exactly the same number. GAMALIEL saved the apostles in a speech of 139 words. FRANKLIN, returning to this country in 1783 after his long and distinguished service in Europe received expressions of congratulation and gratitude from various organizations. His replies to these addresses rarely exceeded 150 words, and one of the most frequently quoted is scarcely one hundred.

SHAKESPEARE appears to have had a very definite conception of what an audience will stand and enjoy. Examination of his best known plays shows strict regard for brevity in all scenes in which an audience of even a few people is present. In the court scene in the "Merchant of Venice," the Duke's longest speech is 135 words, ANTONIO's 149, SHYLOCK's longest 229, PORTIA's 173. In situations which call for longer speeches, SHAKE-

* First published in *Science* 64: 597-598 (1926).

SPEARE is careful to see that they are broken into short units. In "Hamlet," the recitation given in part by HAMLET and in part by the First Player, only 435 words in all, is twice interrupted by POLONIUS, once with the remark, "This is too long." BRUTUS' speech, after the assassination, in "Julius Caesar," is but 348 words in length, and is twice interrupted, the longest unit being 235 words. MARK ANTHONY follows with a speech of less than eleven hundred words, which occupies, as delivered by Mr. JOHN ALEXANDER, just eleven minutes. Yet it is interrupted a dozen times and the longest fragments are but little over 250 words in length. Nowhere, in these three plays at least, does SHAKESPEARE permit a character to address an audience, without interruption, for more than three hundred words.

Since politely suggested "time limits" have not always controlled our after-dinner speakers, is not the advisability of an absolute rule forbidding talks of more than three hundred words indicated? Our speakers could not urge that their messages are too important for such brevity. Who among them chooses a theme more lofty than PAUL's, is weighed with responsibilities graver than LINCOLN's or brings to us a wealth of experience greater than FRANKLIN's? Nor could they maintain in extenuation of their prolixity that these great men had many opportunities for speech-making. GAMALIEL's immortality was gained by one speech, which bears the final stamp of approval, "And they agreed unto him."

Possibly, however, so exact a rule might be construed as a tyrannical limitation of Anglo-Saxon freedom of speech; in which case it might be possible to print on menu cards at all dinners for which formal talks are planned the following instructions for toastmasters and after-dinner speakers of all ages taken from the book of Ecclesiasticus. These directions occupy, it will be noted, in the English translation just 112 words.

Have they made thee ruler of a feast? be not lifted up, be thou among them as one of them; take thought for them, and so sit down. And when thou hast done all thy office, take thy place, that thou mayest be gladdened on their account, and receive a crown for thy well ordering. Speak, thou that art the elder, for it becometh thee, but with sound knowledge: . . . and display not thy wisdom out of season.

Speak, young man, if there be need of thee; yet scarcely if thou be twice asked: sum up thy speech, many things in few words; be as one that knoweth and yet holdeth his tongue.

* * * * *

The foregoing essay has been the subject of more general and wider commendation than anything else I have written. It seems, however, to have been carefully read but rarely. The usual conclusion has been that the article advocated a gag rule permitting no one to speak on *any* occasion, more than three hundred words. Reexamination of title and contents will make it clear that what was said applied only to after-dinner speeches, certainly a special class. Moreover, any definite limitation in number of words was specifically rejected. It is true that the great speeches cited, those of PAUL, GAMALIEL, FRANKLIN, LINCOLN, and certain of SHAKESPEARE's characters, were less than three hundred words in length. The main thesis that a short speech may be, indeed often is, a good speech, still stands.

— VI —

THE FAD AS A FACTOR IN BOTANICAL PUBLICATION*

IF, AS POPE and many others have asserted, "the proper study of mankind is man," botanists may occasionally study botanists and no apology is needed for asking this botanical society to direct its attention to one of the manifestations of botanical psychology. It is obvious that all we know about plants comes to us through the medium of the botanical mind, and in studying the botanical publications of any period it is important to know what botanists were thinking about at that time. For, much as we may dislike the idea, we must admit that the conclusions which investigators draw from their observations, perhaps even the observations themselves, or at least the kind of observations they are most likely to make and to publish, are influenced by what others are observing, publishing and talking about.

In the work of the Plant Disease Survey we deal constantly with observations made by others, and in an attempt to study the relative incidence of disease at different periods it becomes of first importance to discover what particular diseases were in fashion and thus most likely to be noticed at any given time. It was, then, this practical necessity which led me to spend a good deal of time during the past year in reviewing American botanical literature. Some of the incidental results of this study I wish to discuss tonight. To avoid wearying you beyond endurance I have confined the statistical portion of this paper to the last 50 years, 1881 to 1930, and to the following representative American publications: *Bulletin of the Torrey Botanical Club, Botanical Gazette, Phytopathology, American Journal of Botany* and the botanical material contained in the scientific publications of the U. S. Department of Agriculture, including *the Journal of Agricultural Research* and the *Contributions of the U. S. National Herbarium* in so far as these represent the work of the members of the Department of Agriculture.

I am aware of course of the immense volume of American botanical matter outside of this list and of the still greater volume outside of the United States, but the material chosen is merely illustrative, and I must bear in mind the necessity of having enough people left at the conclusion of this paper to elect officers.

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* Address of the retiring president of the Botanical Society of Washington, D. C. First published in *Science* 75: 499-504 (1932).

The curve[1] which gives the total pages of botanical matter exclusive of reviews and abstracts in these publications indicates an increase so great as to eliminate any question of observational error. From a paltry 259 pages in 1881 we attained in 1929 an all-time American high of 5,284. To be sure, the botanical output, like the stock market, broke sharply in 1930, and the curve of increase seems to lose some of the steepness after 1918 or 1919. Nevertheless, this output, especially when viewed against a background of other special journals, experiment station publications and foreign literature in many languages, presents an aspect which is little less than appalling.

Of what does this mass of botanical print consist?

Its composition varies from year to year as a little study or even reflection will speedily reveal.

SPACE DEVOTED TO VARIOUS LINES OF BOTANY IN THE AMERICAN PUBLICATIONS LISTED IN THE TEXT

		Approximate per cent of total pages given to				
Year	Total pages	Systematic botany	Morphology	Physiology	Ecology	Pathology
1881	259	85	..	..	..	..
1890	640	61	5	2	..	24
1900	1,925	34	9	5	1	33
1910	3,342	33	8	6	6	11
1920	4,437	9	11	18	6	29
1930	3,841	10	13	26	1	41

Taking 1881 and the succeeding decimal years as examples we find that of the 259 pages published in 1881, 85 per cent was systematic botany, including work on local floras. In 1890, of a total of 640 pages, systematic botany made up 61 per cent, pathology 24 per cent, all about diseases due to fungi, morphology 5 per cent and physiology 2 per cent. In 1900 there was a total of 1,925 pages, of which systematic botany occupied only 34 per cent, morphology and physiology 9 and 5 per cent, respectively, while there was 1 per cent of ecology and 33 per cent pathology. The figure for ecology is unusually low and that of pathology unusually high, as 1900 seems to have been an exceedingly favorable year for pathology. Ten years later, out of a total of approximately 3,300 pages, 33 per cent was systematic botany and 8 per cent morphology, about 6 per cent each physiology and ecology and 11 per cent pathology. In 1920, of a grand total of 4,437 pages, systematic botany made up less than 10 per cent, morphology 11 per cent, physiology 18 per cent, ecology 6 per cent and pathology 29 per cent, two thirds of which was about fungus diseases. Last year, out of a total of 3,841 pages, systematic botany had about 10 per cent, morphology 13 per cent, physiology 26 per cent, and pathology showed 41 per cent. The apparent slump in ecology is largely due, no doubt, to the establishment of an independent journal for papers in this branch of botany and zoology.

What causes these differences? Why, for example, do systematic

[1] The paper was illustrated by a series of charts showing the volume of publication on various phases of botany during the period 1881 to 1930. The general trend of these curves is indicated in the text with sufficient detail to be easily followed by any one familiar with American botanical literature.

papers make up only about 10 per cent of the total botanical material which appears in these standard journals? I realize, of course, that many good systematic papers appear outside these series and that the decline is in part relative, but no one will contend that systematic botany occupies anything like the position it did fifty or even thirty years ago. No one will claim either that the necessity for systematic work is past. Nor am I convinced that brains capable of serious taxonomic studies are no longer produced in America. I have heard various reasons advanced to account for this obvious decline, among others, that the systematists have made themselves ridiculous by describing numerous scarcely distinguishable "species" by endless changing of names and bickering about nomenclatorial rules. It has also been argued seriously by competent botanists that taxonomic work has been rendered more or less futile by the concept of evolution and that systematic botany depends for its existence on a belief in the fixity of species. However much weight these considerations may have, it seems to be the fact that taxonomy is out of fashion just as truly, if not quite so completely, as snuff.

The question just raised may fairly be reversed and inquiry made as to why morphology, physiology and pathology did not occupy larger places in American botany in 1881. From a much longer list I will cite a number of discoveries or lines of work published between 1859 and 1869 well within the range of facilities then available in the United States and sufficiently striking to have deflected a substantial fraction of botanical interest but for the existence of strong inhibiting influences.

1859 The "Origin of Species," from which the study of structural adaptations, mechanisms of pollination, distribution of seeds, and related problems received a great impetus in Europe.
1861 PASTEUR's discovery that yeast and several species of bacteria were able to live in the absence of oxygen.
1863 MAX SCHULZE's demonstration of the identity of protoplasm of plants and the so-called "sarcode" of the animal physiologists.
1863 Work of SANIO on the process of secondary thickening of the axis of the Dicotyledons and Conifers.
1864 DE BARY's demonstration of the heteroecism of the stem rust of wheat.
1860–5 SACHS' work on photosynthesis and especially the application of the iodine test for starch.
1865 DARWIN's study of climbing plants.
1866 SACHS' great work, "Experimentalphysiologie der Pflanzen," was published. Yet plant physiology did not get fairly under way in the United States until about 1890.
1867 HILDEBRANDT as a result of crossing yellow and a dark brown race of maize noticed xenia, although he did not call it by that name.
1868 The first edition of SACHS' "Lehrbuch."
1868 WILLIAMSON's first memoir on the coal measures.
1869 DARWIN's work on heterostylism.

To the question as to why physiology was not more vigorously pursued in the United States fifty or sixty years ago, one is tempted to repeat that it was out of fashion at that time. Possibly the Victorian botanical mind recoiled instinctively from the study of the life processes and sexuality of plants. Speaking more seriously, the influences which kept American botany in the face of all this distraction almost exclusively on the single track of the taxonomy of flowering plants must have been profound. First of all, of course was the exploration urge. We were then at the period

when the finding of new things in the field took precedence over all other activities, but together with this, emphasizing it and, I believe, extending the period, was the influence of ASA GRAY, who, for a period of over thirty years dominated American botany more completely than any one botanist is likely to again. So complete was this domination that even the systematic study of the lower forms of plant life was almost crowded out of the picture.

The story of the change from this condition, which you may designate as the emancipation from the fetters of the herbarium or, if you prefer, the degeneration of American botanical science, is perhaps more easily traced in the graphs showing pages of publication. And this may be a good time to emphasize again that I am not discussing facts but conceptions, not value of publications but volume of publications.

Physiology, morphology and pathology are all represented, although, of course, very scantily in the first year included in this review. The first to expand markedly was pathology. The flood of papers on plant pathology, especially on diseases due to fungi, followed closely on the perfection by KOCH of the plate method of isolating bacteria and fungi, and the discovery by MILLARDET in France, of the effectiveness of copper and lime as an agent in the control of downy mildew of the grape. Whether we have passed the crest of the curve of production of print on fungus diseases is, of course, problematical. But no doubt some of our colleagues would warmly welcome such a change and there are some indications of its approach. It may be a sign of the times that whereas we used some years ago to hear from mycologists much of the importance of mycology to phytopathology, the summer of 1930 found them insisting at Cambridge that mycology was entitled to consideration in its own right wholly independent of plant pathology.

The story of the rise of physiology, morphology and cytology in the United States is largely the story of the importation, belated importation, perhaps, and the development here of botanical conceptions and methods already under way in Europe. In 1888 or 1889, both physiology and morphology entered on a period of expansion which became more marked nine or ten years later. Ecology claimed a place in the American sun in the year 1899, three years after the publication of WARMING's great work. The "Sem Bot" of the University of Nebraska was engaged in a study which would have led to ecology before 1892, but the cumbersome term they employed, "phytogeography," stood no chance against the shorter term, "ecology."

However, I am less concerned now with these large waves of interest than with the smaller wavelets which may fairly be designated as fads. I might point out, for example, that in the control of plant diseases we have passed through, during the last fifty years, a Bordeaux period, a lime sulfur period, a dusting period, and are now in an eradication and quarantine period. Of course Bordeaux and lime sulfur are still used, but they are no longer talked about. Our most recent pathological fad is obviously that on virus diseases which was launched by the work of ALLARD, 1913 and 1914, and reached what may be a crest in 1926.

Nor are other branches of plant science free from the influence of fads. Among American morphological publications during the period under dis-

cussion, two conspicuous fads may be mentioned. The embryo sac fad which began about 1894 or 1895 and continued to show considerable activity through 1916, but is now apparently practically over, and the chromosome fad, which got fairly under way in 1897 and 1898 following STRASBURGER's great generalization regarding the different numbers of chromosomes in the two generations of a plant, slumped to almost nothing in 1916-17-18, and is now enjoying a second run of popularity due apparently to the discovery that there is a connection between the number of chromosomes and the possibility of producing fertile hybrids in certain genera.

In recent physiological papers the most noticeable fads, at least to the outside observer, are the study of hydrogen-ion concentration and the study of light relations. This last started by the publications of GARNER and ALLARD on the relation between duration of daily illumination and reproduction of certain plants.

Are these Waves of Interest Fads?:— WEBSTER defines fad as a "hobby, whim, custom or amusement followed for a time with exaggerated zeal."

The International Dictionary elaborates this somewhat and defines a fad as "a trivial fancy adopted and pursued for a time with irrational zeal; a matter of no importance, or an important matter imperfectly understood, taken up and urged with more zeal than sense." I leave you to judge of the correctness of the term, but I can find no better English word.

What Starts a Fad?:— In the first place it seems to me, that in order for a real fad to start the stage must be set. Apparently there is no great chance of a fad taking hold close on the heels of another one. It must wait until the collective botanic mind, or rather a portion of the botanic mind, has reached a condition approaching saturation. Note, for example, that the virus disease fad came to the relief of the phytopathologists, as fungus disease publication was approaching 900 pages a year and that ecology came into the United States when taxonomic work was getting close to 1,000 pages a year. Ecology seemed to offer a man with systematic instincts a chance to do a little systematic work without being too critical about the literature and synonomy, and to do some local flora work under a new and attractive name.

Given a favorable stage setting, the fad seems to be started by some discovery, paper or suggestion which is sufficiently different from the common run of good botanical matter to attract attention, but not sufficiently different to prevent its being readily understood. I may say here, that I find the name of but one man connected with the inception of more than one fad. This is, of course, our fellow Washington botanist, H. A. ALLARD. I am credibly informed also that ALLARD is responsible for starting fads in the study of the synchronization of the stridulation of certain insects and the flashing of fireflies. Needless to say, I await with interest what this original mind will start next.

The Next Fad:— I will not even venture to predict what the next fad will be. If I knew I should get out a paper on the subject immediately. A year ago I felt that we were well on our way to a fad for the study of peat bogs by the method of pollen analysis. But this, perhaps because it savors of fossil botany, seems to be thriving better in Europe than America.

Some Potential Fads that Failed to Materialize:— It is, of course, perfectly possible for an outstanding, even a striking, achievement which attracts a good deal of attention to fail to produce a fad. I have never understood why BLAKESLEE's discovery of sexuality in the mucors did not start a fad. "Physiologically balanced solutions" and "antagonism," which were words to conjure with in 1906 and 1908, seem not to have caught the botanical imagination as did "length of day." "Carbo-hydrate-nitrogen-ratio" apparently started no such fad as did "hydrogen-ion concentration." One might reasonably have expected the outstanding success of Dr. COVILLE in developing the native blueberry and thus creating a new industry in the "barrens" of New Jersey to have aroused great interest in our uncultivated native fruits, but there seems to have been no great increase in such interest.

There are apparently certain lines of botanical investigation which have never taken very vigorous root in American soil. Notable among these is, of course, paleobotany, which, in spite of a few very distinguished workers and a wealth of available material, has never assumed a large place in American botanical work.

What Stops a Fad?:— The real answer to the question of why fads stop is that they do not, they merely cease to be fads, that is, each of them leaves some more or less permanent imprint or, to change the figure, influences the course of botanical thought. On the other hand, we certainly do lose most of our interest in subjects before they become exhausted. One explanation which Professor S. W. WILLISTON used to urge with some heat was that it was easier to pick up a new line than to master the literature of an old one. Twenty years ago he predicted that ecology would become as unpopular as taxonomy as soon as the literature was sufficiently voluminous.

You have all heard the students of taxonomy, both animal as well as plant, express grief, even exasperation, that so little attention is paid to their work. I can see no help for this. Neither last summer could the owners of "Tom Thumb" golf courses find any way to bring back the patrons who thronged their courses in 1930. I would urge those who find themselves almost deserted in a no longer fashionable field of botanical effort to cease railing against fate for, to quote Justice HOLMES:

> The law of fashion is a law of life. The crest of the wave of human interest is always moving, and it is enough to know that the depth was greatest in respect of a certain feature or style in literature or music or painting a hundred years ago to be sure that at that point it no longer is so profound. I should draw the conclusion that artists and poets, instead of troubling themselves about the eternal, had better be satisfied if they can stir the feelings of a generation, but that is not my theme.

The Dangers of the Fad:— The dangers of the fad are obvious. All investigators are possessed more or less with what some one has called "the devil of one idea." When a group is so possessed we get something very like mob psychology, which results in an inevitable bias in observation and publication. Illustrations of this will occur to each of you. Many of you will recall—at least from the reproductions in early editions of WILSON's book the "Cell"—that GUIGINARD described and illustrated centrosomes in the lily. It was soon apparent, or at least generally believed, that no such structures exist, but under the impulse of the fad for centrosomes they appeared real enough to this cytologist.

Some of you will recall or have noted in the literature that Dr. T. J. BURRILL, honored wherever pathology is studied as the first to demonstrate the possibility of bacterial disease in plants, described,

Micrococcus toxicatus, Burrill. Cells globular, single and in pairs, rarely in chains of several articles; .00002 in. in diameter; movement oscillatory only.

This organism he believed to cause the poisonous principle of species of *Rhus* and to be capable of penetrating the human skin and inducing the peculiar inflammation which takes place. So enthusiastic was BURRILL over this imaginary discovery that he published it in three different places. Those who in 1922 saw the unrestrained enthusiasm of the recognized leaders of plant pathology over the preliminary and possibly mistaken announcement of certain organisms in the cells of plants affected with mosaic diseases were witnessing no new phenomenon. Similar enthusiasm if I may judge from the literature, greeted the alleged bacteria in poison ivy forty years earlier. In one of the great speeches of all time, PAUL, standing on Mars Hill, is said to have addressed an audience "who spent their time in nothing else but either to tell or to hear some new things." This is a condition not wholly foreign to other audiences in other times, even American botanical audiences.

The danger in this sort of publication is, however, more apparent than real. A wise and tolerant botanical public, realizing that these great investigators were acting under the impulse of "fad psychology," soon forgets these slips and the careful reader of tomorrow, noticing in the pathological literature of 1915 to 1925 an exceedingly large number of reports of virus diseases, will recall that virus diseases were all the rage in those days and will take these reports with more than a grain of salt.

Just as a man in good general health who consulted a physician between 1910 and 1920 was predestined to be diagnosed as having appendicitis, so a plant which showed any unusual abnormality between 1915 and 1925 was sure to be under suspicion of having some mosaic disease. Five years ago we were busily studying two diseases of strawberries which we regarded as of virus-origin. One has since turned out to be caused by nematodes and the other is apparently a genetic variation.

The Advantages of the Fad:— A year ago I was convinced that fads, at least botanical fads, were an almost unmitigated nuisance. Somewhat more mature reflection, however, serves to convince me of the contrary. I now regard them with a toleration which approaches enthusiasm.

Something like a fad may be necessary to jar the human mind, even the botanical mind, from its old moorings. To cast out the devil of one idea from the botanical mind is often a decided advance. It may be that occasionally, when the devil of one idea is cast out he will return, and finding the botanical house swept and garnished, take seven other spirits worse than himself and enter in and dwell—which might appear worse. On the other hand, it may really be better, and at any rate they will not stay. New fads are often better than the old. I am glad for example, that the fad for chewing tobacco has been replaced by the cigarette fad. I view with something very like dismay the possibilities if the ladies had all taken up tobacco during those earlier days. With almost equal dismay I view the possibilities if changes in American botanical interests had ceased at, for example, the embryo sac stage.

It may well be that the fad offers the only way to really introduce a new concept into the botanical world. By this I do not mean merely to get the idea into literature, but to get it into botanical thinking.

In his "Leaven of Science" Sir WILLIAM OSLER cites the following story, told by Sir ROBERT CHRISTIAN, about BARCLAY, one of the leading anatomists of the early part of the nineteenth century. BARCLAY spoke to his class as follows:

> Gentlemen, while carrying on your work in the dissecting room, beware of making anatomical discoveries; and above all beware of rushing with them into print. Our precursors have left us little to discover. You may, perhaps, fall in with a supernumerary muscle or tendon, a slight deviation or branchlet of an artery, or, perhaps a minute stray twig of a nerve—that will be all. But beware! Publish the fact, and ten chances to one you will have it shown that you have been forestalled long ago. Anatomy may be likened to a harvest field. First come the reapers, who, entering upon untrodden ground, cut down great stores of corn from all sides of them. These are the early anatomists of modern Europe, such as VESALIUS, FALLOPIUS, MALPIGHI and HARVEY. Then come the gleaners, who gather up ears enough from the bare ridges to make a few loaves of bread. Such were the anatomists of last century—VALSALVA, COTUNNIUS, HALLER, VICQ D'AZYR, CAMPER, HUNTER and the two MONROES. Last of all come the geese, who still contrive to pick up a few scattered grains here and there among the stubble, and waddle home in the evening, poor things, cackling with joy because of their success. Gentlemen, we are the geese.

OSLER's comment on this story is:

> Yes, geese they were, gleaning amid the stubble of a restricted field, when the broad acres of biology were open before them. Those were the days when anatomy meant a knowledge of the human frame alone; and yet the way had been opened to the larger view by the work of JOHN HUNTER, whose comprehensive mind grasped as proper subjects of study for the anatomist all the manifestations of life in order and disorder.

To OSLER's comment I beg leave to add that probably only by strength of interest in various fads were the geese called away from their gleanings and but for the widening of interest induced by fads they and their successors might well have remained in the stubble.

— VII —

BUREAUCRACY AS A WAY OF LIFE*

THE government worker lives in a glass house—his hours, his pay, his tasks are known or may be known to all. As a dweller in a glass house he is by proverb prohibited from throwing stones. On the other hand, he has been for years the target for two general, almost blanket, criticisms—first, that he is an inefficient workman and, second, that he is a moral weakling.

Recently less has been said about the inefficiency of government workers. This may well be because of the drastic deflation undergone in the standing of our leaders of industry, trade and banking. On the other hand, certain by no means to be despised voices have been raised to the effect that the government worker is at least as efficient as, perhaps more efficient than his brother in industry. Witness for example the statement of OSWALD GARRISON VILLARD:[1]

> I have not been moved to call attention to Mr. EASTMAN because he is exceptional; there are many other public officers who are serving the government with great ability and devotion. He himself answers the question whether it is possible for a government to enlist men of first-class competence and shining integrity without paying them the high salaries offered by private corporations to the men they select for president or vice-president. When I contrast the character and talents of Mr. EASTMAN with those of some of the men who have been paid a million dollars a year by banks and steel companies, it is to laugh. . . . I have no doubt that he could have wangled a fat job for himself from some of the large corporations years ago, and feathered his own nest most richly. He has preferred his small Government salary and the privilege of serving his fellow-countrymen, which is delightful proof that the private-profit motive is not essential to the carrying on of a civilized government. And there are many like Mr. EASTMAN.

In the same vein we have the thoughtful statements of CHARLES and WILLIAM BEARD in "The Case for Bureaucracy":[2]

> It would be easy to pick out illustrations of steady and efficient functioning on the part of numerous bureaus and agencies in Washington—work done by the bureau of mines in saving human lives, by the coast guard in stormy seas winter and summer, by the men who manage the vast system of airways, by the forest service in conserving and guarding the national forest domain, by the public health service, and so on through a catalogue filling a volume. Where we find a bureau functioning in some

* First published in *Science* 83: 497-499 (1936).

[1] *Nation,* February 7, 1934.

[2] *Scribner's Magazine,* 93: 4, April, 1933.

field that does not invite collision with private enterprise, we usually discover the most intelligence and public spirit. But generally the bureaus are hampered in constructive work by acquisitive pressures from the outside. . . .

With more direct reference to the scientific work of the government, R. L. DUFFUS, writing in *Harper's* for June, 1933, says:

Consider the scientific agencies which can be found in almost all the departments. They are full of men who are building roads into the future. In stuffy little offices, in laboratories smelling of chemicals and of decaying organic matter, these devotees study the habits of insects, the diseases of poultry, human beings, and livestock; they test soils and seeds, they weigh the earth and the stars, and when called upon as PAUL DE KRUIF has glowingly related, lay down their lives in a rather casual way for the service of mankind.

A somewhat special class, perhaps, these investigators, but for the larger groups also there are to be found defenders. For example, the BEARDS point out that the efficiency of the fire departments of our various cities (manned chiefly by Civil Service employees) is recognized not only by occasional notice of a particular heroism in newspapers, but strange as it may seem by that all-important American document, the balance sheet. Fire-insurance companies make it a practice to lower their rates in cities which have standard equipment for fighting fires. The assumption is that if the men have the apparatus, they will use it effectively. Here business takes the efficiency of government for granted and measures results in dollars and cents.

Secretary of Agriculture H. A. WALLACE, in speaking a year or two ago before a group of Department employees, paid obviously sincere tribute to the fidelity and ability of government workers and the efficiency with which government work is conducted. Referring specifically to the clerical force, he said that he well remembered and had come to agree with the point of view of his father, Secretary of Agriculture in the Harding Administration, who said at that time that he found that federal clerical work was more efficiently conducted than similar work in private business and planned to take some of the federal workers back with him when he retired to private life.

So much for efficiency; now as to morality. Only last week our minister insisted from the pulpit, with cautious reservations in favor of those with intellectual interests, that working for the United States Government is bad for the character. From listening to his sermons on a surprising number of Sundays, I take it that he feels that the particular type of competitive effort supposed to be associated with business and with most of the professions is necessary to the development of real character.

This idea is not new even to the clergy. I have recently read a letter dated October 11, 1850, from the Reverend A. P. CHUTE, then of Lynnfield, Massachusetts, to a brother clergyman in Maine, which includes the following paragraph:

I observe that your bro. Maurice has lost his office at Belfast. My brother who was a subordinate in the Castine office will also lose his office probably, though it is not certain. He is, however, expecting it. It should not be regretted either by them or their friends for office-holding under the Government has a bad effect on men, intellectually & morally. A man long in public office loses his spirit of *self-reliance* if he does not deteriorate in moral character.

(To point out that this same A. P. CHUTE writes in September, 1861,

"At his desk in the Custom House Boston" to his "Dear Brother Blake" would not add anything to the present discussion.)

The agreement of these reverend gentlemen, separated in time by an eighty-year interval, is impressive. Whether they are correct depends on one's ideas of "character" and "morality." This problem is well stated by the BEARDS in the article to which reference has already been made.

> Is the bureaucrat's morality, the job holder's morality, ipso facto, worse than the business man's morality? Who are our leading business men? They are the men who have made the most money. What is the rule of business? It is to buy in the cheapest market and sell in the dearest, to give as little as possible for as much as one can get. If a dreamy professor comes along, meets a realtor, and pays him a thousand dollars more for a house than the latter is willing to sell it for, would not the former be condemned as a fool in any well-bred American community and the latter congratulated on "making a good thing out of the deal?" There are exceptions, to be sure, and much talk about service, but the business of business is to get money, to collect what the traffic will bear. If not, what is it?
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> The justification for this kind of ethics is that it supplies initiative, but peril of it lies in the fact that no civilization can endure which has written over its shrine: No profit, nothing doing. And the justification of the bureaucracy lies in the fact that, allowing for dead wood and dead heads, it supplies from top to bottom an ideal which this country needs, the true soldier's ideal, namely, that great deeds may be done without hope of profit, either near or distant, openly and professed or sneakingly and concealed.

The real problem then seems to boil down to something like this. Is active labor under a profit system necessary to the development of character? Curiously enough, at least one great organization stands fast in the faith that an entire absence of such a motive is necessary to the highest development of character. Our largest church by formal vows makes sure that its leaders shall be free from the distractions of profit seeking and most of our smaller denominations wisely make equally certain by somewhat less formal means of a similar position in their clergy.

The great educational institutions also while carrying on a rather mild competition among themselves on salary schedules have never attempted to compete with business concerns. Curiously, or perhaps naturally, enough, the college professor like the clergyman sometimes sighs for the degeneracy of his brother in the Civil Service of the United States.

In a biographical sketch of the late ROLAND THAXTER of Harvard, his successor recounts a discussion of possible openings in which Dr. THAXTER remarked with a sigh, "Of course one can always get some sort of position in Washington, and even able men like Dr. LYMAN seem to like it there, I'm sure I don't know why!" Now the answer is simple. Even able men "like it" in the Civil Service because it enables them to do effective and useful work among pleasant people who are companionable and understanding.

Dr. GEORGE SARTON, who is spending his life in the study of science in relation to human life in the past as well as in the present, points out the relation of surroundings to the deepest happiness in the following phrase:

> To be truly happy and gay we must be able to pursue the truth, not alone, but among lovable men and women, who are kind to us and to whom we can show our own kindness. Even as the discovery of any particle of truth, whether it be to our advantage or not, pleasant or unpleasant, is a positive gain for the whole world, even so every act of kindness is a creation in the right direction.

Our tactful family physician says that our village is a pleasant place in which to live because it contains a large per cent of men who have been very careful in the selection of their wives. Without attempting to defend this somewhat startling generalization I will adopt his phraseology and assert on the basis of over twenty years of work in its ranks that the Civil Service is a pleasant place to live and work because it is made up of carefully selected people; selected not so much by the formal tests as by the type of work they do, the type of life they may be expected to lead, and the almost entire absence of a profit motive. I believe them to be distinguished among American groups for courtesy, generosity, industry, honesty and happiness. Whether the characteristics which I find among my associates are to be rated as good or bad I must leave to the clergymen to decide. That is their field of specialization—not mine. But I have the feeling that they bear strong resemblance to the ideas held out as ideals by certain great moral leaders of different ages.

— VIII —

THE EXCESSIVE MEEKNESS OF AMERICAN BOTANISTS*

THE meekness of American botanists has been so long and so generally recognized that no comment was offered and certainly no surprise was occasioned when some years ago SEIFRIZ[1] quoted a visiting Swiss botanist as calling attention to the fact that it is easy in most American universities to recognize the botany building, because it is the "oldest building to be seen anywhere." We have, however, taken a certain satisfaction in the feeling that our zoological friends were ready to assert themselves, to think and to act independently, and that thus a fair balance would be maintained, and the general field of biology adequately, if not evenly, cultivated.

Rather recently, however, there has appeared in various quarters the more disturbing suggestion that the apparent forcefulness of zoologists is relative only to their botanical associates and that biologists as a group are inclined to be meek and to accept their basic theories and even their methods ready-made and second-hand; moreover, that these hand-me-downs do not really fit. For example, WHITEHEAD[2] says, ". . . at the present moment, the prestige of the more perfect scientific form belongs to the physical sciences. Accordingly, biology apes the manners of physics. It is orthodox to hold that there is nothing in biology but what is physical mechanism under somewhat complex circumstances." To which RUSSELL[3] adds, ". . . Biology, impressed by the success of physical concepts in their own sphere at the time of the great development of the classical mechanics, took over to itself concepts and methods which were clearly inappropriate and inadequate."

Now criticism of this sort is disturbing enough, but after all, we have been believing that most of the workers in any field need not concern themselves with fundamental theory—provided, of course, they are doing their daily duty of accumulating "facts" with proper zeal and appropriate methods. Even this haven of refuge seems now endangered from two distinct angles, one, that the very volume and variety of the accumulated facts make real comprehension more difficult; and the other, that our present methods

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* First published in *Science* 85: 580-582 (1937).

[1] *The Scientific Monthly,* May, 1928.

[2] "Science and the Modern World," p. 144.

[3] "The Interpretation of Development and Heredity," p. 163.

are as badly suited to our needs as our present concepts. On the first point, witness CROWTHER,[4] "But the neglect of comprehensive synthesis by which all the facts could be ordered led to intellectual chaos, just as the blind drive to increase production of goods, without working out any comprehensive system of distribution, led to chaos in social life," or LYON,[5] who says the same thing in medical rather than economic figures. "There is a serious side to this unabsorbed gorge of science. It has given our people a bad indigestion. It lies in the public stomach and troubles their dreams. They do not know enough to know good science from bad." And on the second, SULLIVAN[6] says: ". . . Discrimination is fatiguing; also it makes appeal to sensibilities which many earnest 'scientific workers' do not possess. It is much easier to make measurements than to know exactly what you are measuring. To give up the ideal of measurability would be the equivalent, to many people, of abandoning 'science' altogether. 'Science is measurement,' we are told. . . . In their eagerness to measure something, our researchers seem to lose their ordinary common sense, whereas their subject really requires the subtlety and sympathy of a very good novelist."

This last challenge is particularly disturbing to our complacency, since it at least suggests that the real foundation of our recently acquired faith in supposedly exact measurement may be found in mental laziness. For botanists—American botanists at least—have at last discovered mathematics and appear cheerfully ready to abandon any form of inquiry or information gathering which does not readily lend itself to measurement and statistical analysis. Like all good converts, we are trying to be more orthodox than the Pope, for the mathematicians are quite ready to concede that there are in biology important fields of inquiry where mathematics can play little part. To quote CARMICHAEL[7]: "It must also be remembered that there are important Chapters of Science which do not come readily under the domain of number. Witness much of biology and in particular the theories of phylogenetic development." Some would go even further. WHITEHEAD[8] agrees with HENRI POINCARÉ in insisting "that instruments of precision, used unseasonably, may hinder the advance of science."

Certainly, within the writer's field of study (plant diseases) there occur phenomena which appear to defy accurate measurement by present methods, yet which seem important and abundantly worthy of record. Reference is here made to the fluctuations in plant disease which are of very great biological interest and economic importance, but which are most inadequately recorded, largely because they do not appear to be readily measurable. That large differences occur no one denies. That they are hard to measure is granted. The essential difficulty of the undertaking is emphasized whenever the accurate measurement of losses from a single plant disease is undertaken. One of the most interesting recent attempts[9] is based on a comparison of the yields of adjacent smutted and smut-free plants of dent corn. In spite of the care used in the field work and in the analysis of results, this

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[4] *Soviet Science.*

[5] *Sigma Xi Quarterly,* December, 1936, p. 208.

[6] "Gallio—or the Tyranny of Science," p. 50.

[7] *The Scientific Monthly,* December, 1935, p. 495.

[8] "Adventures of Ideas," p. 311.

[9] I. J. JOHNSON and J. J. CHRISTENSEN, *Phytopathology,* 25: 223-233, 1935.

work is still open to the criticism that the diseased plants may have become infected because they were different in the first place.

However, a discussion of the methods of measuring disease losses does not belong in this paper. There is no present possibility, even if we had developed the technique, of making measurements of losses due to even the important known diseases of our agricultural crops. Largely on this account, there is a decided, possibly an increasing, reluctance on the part of plant pathologists to record these differences at all. This willingness largely to ignore for purposes of record anything which can not be measured and set down in mathematical terms is, of course, just one more manifestation of the excessive meekness of American botanists. There may well be biological phenomena, the *record* of whose occurrence is more important than their measurement and which should be recorded even if they can not be measured. For example, only a few years ago, 1931, eel grass (*Zostera marina*) was common in the shallow waters of the Atlantic seaboard, from North Carolina to Nova Scotia—now it is rare. Its diminution was so sudden that no opportunity was given for statistical study, even by the quadrat method. Yet obviously the biologists of the future are entitled to the information that a striking phenomenon occurred in our coastal waters at this period, even if we are not able to furnish figures.

This is, of course, an extreme case, but somewhat similar situations arise over and over again in our consideration of the variations in the incidence of plant diseases. For example, it is biologically and economically important to know that bacterial wilt was exceedingly abundant on sweet corn in the Hudson Valley of New York in 1932 and 1933 and very rare in 1934 and 1935, but whether the loss occasioned in the earlier years was 20 or 40 per cent and whether the loss in the two later years was one half or three fourths of 1 per cent is of merely academic interest. In 1932 and 1933 the losses were disastrous, and in 1934 and 1935 negligible.

Nothing could be further from my thoughts than to suggest any radical reform such as would be needed to alter our general professional attitude or develop new concepts and methods particularly suited to the study of living things. I certainly cherish no illusions as to the possibility of securing some slowing down of the rate of accumulation of observations or even a little breathing spell during which we might consider what, if anything, these accumulated facts signify. Quite the contrary, I propose merely that we students of living things shall not restrict ourselves to the type of observation or record prescribed by devotees of other branches of science, but shall record as clearly as we may whatever phenomena seem interesting to us, even though we can not measure them with great accuracy. For such unrestrained self-expression, Dr. SARTON has recently furnished an adequate slogan in his book, "The Study of the History of Science"—"No scientist worth his salt has ever abandoned an investigation simply because the attainable precision was too low."

— IX —

DISEASE DAMAGE AND POLLINATION TYPES IN "GRAINS"*

AMERICAN students of plant diseases for over half a century have been more concerned with the organisms causing disease, especially fungi, than with the host plants. No doubt the present interest in plant breeding will tend to readjust the balance. But even today interest in virus diseases centers rather on the nature of the viruses than on their effects. In particular, any attempt to generalize as to the disease relations of groups of plants has been almost wholly lacking from our literature. HARTLEY'S[1] discussion of the disease hazards incident to planting clonal varieties of trees is a conspicuous exception. He notes that "The expectation that genetic uniformity will favor the building up of specialized strains of parasites is supported by practical experience with such clonal cultures as Lombardy poplar avenues, rubber plantations, fruit trees, roses, potatoes, bananas, sugar cane and the creeping-bent golf-green grasses." The present paper is an attempt to examine some of the available evidence in order to determine whether such a relation is observable among major crop plants.

That numerous biological strains of many parasitic fungi exist in nature and that they vary continually through crossing and otherwise has been abundantly demonstrated in recent literature. Some of our crop plants, on the other hand, because of the method used in propagation or their own floral characteristics, have very much less natural opportunity for variation and adaptation than others. It seems probable that in their long-continued mutual association, parasites might well obtain a relatively greater advantage over those host plants which themselves had the least capacity for variation and adjustment. This might express itself in greater disease losses over a period of years or, in the case of parasites particularly favored by special environmental conditions, it might express itself in epidemics in the relatively weaker groups of host plants.

As to the capacity of the host to vary and adjust itself, vegetatively propagated plants would be less efficient than those produced from seed. Among plants grown from seed there would be a gradation in this respect from plants largely self-pollinated, to plants with perfect flowers which are usually cross-pollinated, then monoecious and finally dioecious or hetero-

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*First published in *Science* 89: 339-340 (1939).

[1] CARL HARTLEY, *Phytopathology*, 29: 9, 1939.

stylous plants. Of course, no such complete series exists among comparable crop plants, but those commercially classed as "grains" offer some interesting contrasts.

In an attempt to evaluate disease losses in the United States as a whole, one naturally turns first to the estimates of disease losses compiled by the Plant Disease Survey. These have, however, been systematically collected for only twenty years and suffer, to some extent, from the lack of regular reports from many states. In fact, there are no subjects on which present-day plant pathologists are more reluctant to express an opinion than the extent of crop losses from disease and the economic importance of plant diseases. These are obviously not the same thing. Economic importance, while difficult to measure, must be in some way a function of the value of the crop concerned, the loss caused by disease and the fluctuations in loss. This last is a very important consideration. Other things being equal, even the average losses over a period of years, that disease is the most important which fluctuates most. Secretary WALLACE has said, "Fluctuations in yields cause as much embarrassment as unbalanced acreage."[2]

In searching for some means of measuring the relative importance of diseases of economic plants, it dawned upon me that volume of publication must, in some degree at least and for the more important crops, express the opinion of plant pathologists and others interested as to the importance of diseases.

I have accordingly tabulated the total pages regarding the diseases of various important crops in the publications of the U. S. Department of Agriculture up to January, 1925, of the Experiment Stations up to December 1, 1927, and in *Phytopathology* up to January, 1927. This covers, for the experiment stations, a period of 40 years and, in the case of the Federal Government, goes back even before the Department of Agriculture was organized and includes some publications of the Commissioner of Patents. My reason for stopping at a point over a decade ago is that these are the dates of the excellent bibliographies compiled by Miss JESSIE ALLEN, librarian of the Bureau of Plant Industry. In the introduction to one of these, Miss ALLEN states that, "Some appraisement has been made for subjects upon which there are many contributions, the brief and less important ones being omitted. For subjects on which there are few publications all have been included." Thus any error in the figures is in increasing their size for the less important crops or diseases.

Obviously such a means of measuring the importance of plant diseases can have no validity in the case of many small and highly specialized crops where the publications of a small group of enthusiastic workers or even one investigator—or for that matter a single paper—easily assume undue importance. Nor could we expect to compare too closely, succulent vegetables with grains. But it may be possible to obtain a measure of the apparent relative economic importance of diseases in the culture of crops which have a not too widely different value per acre, are marketed in somewhat the same way, and produced by more or less comparable groups.

Such a unit is apparently found in the crops classed together as "grain crops" for statistical purposes in the publications of the U. S. Department of Agriculture. For such crops this means of measurement must have

[2] *New Republic*, December 2, 1936.

real significance, unless there has been something radically wrong with the administration of plant disease work in this country over a period of half a century. Most of this work has been tax-supported and the obligation to see that most of the money was spent where most needed has been generally recognized. Indeed, it would probably have been enforced by popular pressure.

If such figures are to be used as a means of evaluating the relative importance of diseases in different crops, some adjustment must be made for the value of the crop; the most obvious method seems to be to divide the number of pages published by the value of the crop concerned in millions of dollars. This has been done, using the average value of the crop for the ten-year period 1910-1919 as a basis of computation. Several other periods were tried with no difference in the order of the various crops. Some of the results of this summary are given in the following table.

Relative Economic Importance of Disease in Various "Grains" as Indicated by Volume of Publication in U. S. A.

	Total pages	Disease index (corrected by value of crop)
Flax	426	14.2
Rice	205	4.9
Barley	526	3.5
Wheat	3,526	3.4
Sorghum	305	2.3
Oats	1,178	1.8
Rye	94	1.5
Corn	1,941	0.8
Buckwheat	0	0

For comparison with these crops, it may be noted that the disease indices computed on the same basis for grapes and the important tree fruits—all vegetatively propagated—are over 30 and that for potatoes over 20.

Whatever may be one's opinion of the validity of this method of appraising the importance of diseases in the culture of a crop, there are probably few who will take exception to the fruits being placed far above the grains in this respect or to the order of most of the "grain" crops in the table.

In this it is at least worthy of comment that the highest six are largely self-pollinated under natural conditions, rye and corn chiefly cross-pollinated, and buckwheat heterostylous and thus always cross-pollinated.

There can be no point in emphasizing too much the fact that at least up to 1927 no single page had been devoted to diseases of buckwheat in the literature reviewed. Indeed, the only reference to the subject found so far is the statement in Robbins and Ramaley's text,[3] "It is singularly free from insect pests and fungous diseases." To be sure, buckwheat is not a major crop, nor on the other hand is it negligible. Its average farm value per year for the period 1910 to 1919 was over 16 million dollars, and in 1920 the farm value of the crop in New York State was over 6 million.

[3] "Plants Useful to Man," p. 184.

Serious epidemics of disease in crops valued at 6 millions have not gone unnoticed in New York State during the past 25 years.

Any one who is unwilling to accept the significance of a correlation between the striking freedom from disease and the fact that the plant can reproduce only by crossing (a condition comparable to that in all the higher animals) should at least advance some other hypothesis.

— X —

BOTANICAL RESEARCH BY UNFASHIONABLE TECHNICS*

NEARLY two years ago in suggesting that among certain crops in the United States there was a relation between disease damage and pollination behavior, I ventured to use volume of publication on the diseases of these crops in relation to their farm value as a measure of the commercial importance of diseases in their culture.[1]

The main thesis that, at least among the crops classed as "grains," disease losses are much more important in those groups which are wholly or largely self-pollinated than in those which are largely cross-pollinated, has not yet been attacked. Indeed, if we are to take the teachings of the breeders seriously and actually credit the existence of selection as a vital force rather than just something in a book, it could hardly be otherwise.

On the other hand, a good many have found a source of amusement in the type of evidence offered. A characteristic comment has been, "this is interesting but not very scientific." Now, when some one makes the comment of "interesting but not very scientific," I find myself somewhat in the position of JOHN WESLEY regarding music. When some of the orthodox complained that the songs which thousands of his followers were singing throughout England were not sacred music, he replied with the question, "Why leave all the good tunes to the devil?" Why should we in our search for evidence on live problems leave all the interesting fields to the economists?

The general thesis advanced is, in view of the present great interest in breeding for disease resistance, of real, even basic, importance. Such generalizations are dangerous enough and hard to prove, but the available evidence should at least be considered on its merits and without any attempt to find too difficult an explanation. It is in dealing with just such general observations that present-day biologists lay themselves open to the criticism once leveled at a well-known writer by the late NEWTON D. BAKER that he "discards the obvious as unreasonable and embraces the unreasonable because it is not obvious."[2]

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* Address of the vice-president and chairman of the Section for the Botanical Sciences of the American Association for the Advancement of Science, Philadelphia, December 29, 1940. — First published in *Science* 93: 172-176 (1941).

[1] *Science,* 89: 339-340, 1939.

[2] *Yale Review,* 30: p. 40, September, 1940.

In this case the inference drawn seemed to be in line with the general impression among informed agronomists, yet the suggestion was largely disregarded because of the unusual source of the figures presented. This question then has very much broader significance, which is my justification for bringing it up here, a significance as wide as the whole field of biological investigation. For it is only one phase of the more general question, shall we permit easy and fashionable methods to determine our lines of research, or shall we attack interesting and important problems with the best tools we can find or devise?

Since the publication of the earlier article I have examined three other possible sources for evidence on this point and find them in essential agreement. For example, in a series of articles discussing progress and possibilities in plant and animal breeding, prepared by specialists of experience and high standing in their respective fields, and published in the United States Department of Agriculture Yearbooks for 1936 and 1937, relatively much more space was given to discussing disease resistance in wheat, oats and barley, all largely self-pollinated, than in corn, which is, of course, cross-pollinated. The relative amounts expressed as percentage of the total space which was given to this phase were: wheat, 11.5; oats, 10.5; barley, 7.0; corn, 1.9. Moreover, the disease loss estimates compiled and published by the United States Department of Agriculture from figures sent in by thousands of crop reporters in all parts of the United States are in agreement with the foregoing. The estimated average annual reductions for the decade 1916-1925,[3] which are slightly higher than for the earlier years, but fall in the same order, are in percentage: wheat, 5.2; oats, 2.8; barley, 2.7; corn, 0.4. Rye is not mentioned. Finally, the estimates compiled by the Plant Disease Survey for the years 1917-1937, inclusive, show a striking agreement with the foregoing. This, of course, raises the question of the significance of these estimates, or indeed any estimates relating to plant life.

Three years in the Plant Disease Survey followed by four seasons of renewed contact with cranberry growers and their problems led to much consideration of estimates and their place in biology, with conclusions which may be worth sharing, in spite of, or perhaps because of, the fact that they may not agree with current fashionable concepts.

First of all, what is an estimate? An estimate, according to Webster, is a "judgment of opinion, usually implying careful consideration or research—a judgment made by calculation, especially from incomplete data." This definition applies well to both estimates of crops and estimates of crop losses. Comparison of the actual shipments of cranberries by the New England Cranberry Sales Company with the estimates turned in by the members in September, only a short time before the harvest begins, over a period of 24 years showed errors of 5 per cent or over in one or both of the two major varieties in eighteen of the years and errors of 20 per cent or over in six years. Moreover, in most cases the causes of the errors seem to have been psychological rather than observational. The inherent caution of real New Englanders appears in a decided tendency to underestimate the Early Blacks, the variety first harvested. A conspicuous cause of error appears also in the influence of the previous crop. Very large

[3] "Crops and Markets," Vol. 3. Supplement 10, pp. 321-322, October, 1926.

crops tend to be greatly underestimated if they follow small crops and small crops to be greatly overestimated if they follow large crops. May I hasten to add that these serious, demonstrated errors led to no suggestion on the part of the cranberry growers that estimates be dropped. They apparently agreed with POINCARÉ that "it is far better to foresee even without certainty than not to foresee at all."[4]

It may not be impertinent to add that only academic minds question the usefulness of estimates. Businesses are projected, factories built, professions chosen and crops planted on the basis of estimated future needs. Money is loaned, houses are purchased, horses are bought and are (or used to be) swapped on the basis of estimated value. Husbands are selected, married and, with increasing frequency nowadays, also swapped on the basis of estimated present or future worth. Many scientific investigators, on the other hand, only with the greatest reluctance, hazard their reputations (to say nothing of their money) on even the most carefully qualified estimates, preferring to devote their attention to phenomena which may be "measured."

Admittedly—indeed by definition—a high degree of numerical accuracy can not be expected of estimates. If in the case of the crop estimates just cited, a group of highly intelligent and vitally interested cranberry growers dealing with quantities which could be rechecked very soon after they were estimated, made frequent errors of 20 per cent or over, there are probably very much larger errors in our crop loss estimates. The real question is, are they adequate for or at least the best available means of serving their purpose?

The purpose of crop estimates is clearly understood by all. It is to furnish those concerned in handling the crop with the most nearly accurate advance information possible. What is the purpose of the estimates of crop losses? Primarily, I take it, to furnish some basis for the comparison of one year with another, one region with another and one disease with another. So far as can be judged from reading the introductions to the various published summaries of estimates it was recognized from the first that they could not be very accurate. They were not measurements, they were "judgments of opinion." Into such judgments certain psychological factors undoubtedly enter as causes of errors. There has been, I believe, a general tendency on the part of most collaborators to err on the side of caution and to underestimate losses from diseases. This may be in part a reaction from the fantastic estimates of years ago. Certainly in some cases where it has been possible to measure the effects of disease, the figures have been much higher than the usual estimates. Study of the estimates over a period of years leads me to the conclusion that there is some irregularity as a result of special interest or knowledge, and that estimates are influenced by general interest or lack of interest in a disease. Local prejudices certainly seem to influence some of the estimates, and it is charged that there is some deliberate distortion.

It might be no less than fair also to call attention to the fact that estimates differ in degree only—not in kind—from many of our so-called measurements; that there are always subjective sources of error, and that in

[4] "The Foundations of Science" (Authorized Translation by G. B. HALSTED), Science Press. 1913.

general, the larger the problem and the wider the field studied, the greater the probability of error.

It is my impression, gained through several years of study and observation, that the tendency to discredit the estimates of crop losses results from the inclination to expect of them wholly unattainable degrees of numerical accuracy, and that this tendency is fostered by the fact that the estimates are expressed in numbers. If no claim is made to numerical accuracy in the crop loss estimates, why are they numerically expressed? Simply because of the limitations of any other method of expression. The case is admirably summarized by SOROKIN in a discussion[5] of methodology in another field. His arguments, which seem to me unanswerable, are that the numerical method is more concise and economical, and that verbal quantitativism has a very limited number of gradations. A glance at the table which contrasts actual published figures with attempted verbal equivalents, will illustrate the point. After all, we have learned to use figures with discretion in some fields. If a text we use in 1939 states that certain plants grew a million years ago, we do not say in 1940 that these plants grew a million and one years ago.

In view of all their admitted inaccuracies, are crop loss estimates worth while? I believe the answer to this question should be based on two further questions—How important are the problems to which they relate? Are they the best tools available? Obviously they are our only available means of studying the fluctuations in the intensity of many diseases from year to year. Just how important this is depends, of course, on one's judgment of the scientific and practical importance of epidemiology.

ESTIMATED LOSSES DUE TO BACTERIAL WILT IN SWEET CORN IN PENNSYLVANIA

Year	Numerical (percentage)	Verbal quantitative
1931	..	did not notice it
1932	45	a massacre
1933	25	heavy losses
1934	20	less heavy losses
1935	15	still less heavy losses
1936	8	losses still less heavy but still heavy
1937	3	losses light but still noticeable

In this field, at least, I admit myself a prejudiced witness, for I am deeply interested in the history of plant diseases. On the basis of estimates collected by the Federal Government, some of them before the Department of Agriculture was organized, I was able to trace the spread of potato blight in 1843, '44, '45[6] and to publish for the first time a record of the extent of losses from the disease in another great rot period of 1885-1886.[7] I confess myself not greatly concerned by the fact that New York records the loss in 1885 as 38 per cent and adjacent Vermont as 17 per cent, nor do I care whether either figure is accurate. The fact is that we are able to

[5] P. A. SOROKIN, "Social and Cultural Dynamics," Vol. 2, p. 22.
[6] *Jour. Washington Academy of Science*, 23: 435-446, 1933.
[7] *Phytopathology*, 24: 76-78, 1934.

learn from them that the losses in both states were large in 1885—much larger than in the subsequent year.

At some risk to what little may be left of my scientific reputation I wish to add that for a study of the fluctuations of disease from year to year, estimates may be just as useful as figures based on laborious measurements and calculations. A figure published a few years ago[8] gives some information about the incidence of fruit rots of cranberries on Cape Cod during five years. It is based on the results of storage tests of eight to ten lots of the most important cranberry variety in Massachusetts taken from the same bogs and from nearly the same sections of these bogs year after year. The lots were adequately sampled, carefully sorted and accurately counted. For purposes of comparison they show that the crops of 1931 and 1933 were of very poor keeping quality, the crop of 1932 very good, and the crops of the two other years good but not exceptional. These facts were already well known to inspectors, sales agents and growers throughout the area on the basis of their own experience and observation long before my tests were concluded.

There is another and very practical angle to the study of plant disease control in which estimates of disease, yes, and forecasts of the probable incidence of disease, are of direct significance. How else can we evaluate our control measures? I have already quoted more than once the reply I received when a few years ago I asked a plant pathologist who had spent several years in the study of the control of damping-off in forest nurseries for a brief statement of his recommendation for disease control. His first sentence was "If your loss is less than 15 per cent, forget it." I think it would be an excellent practice and would tend to establish us in the eyes of practical men if all our recommendations for disease control began, "If your loss is less than — per cent, forget it." In order to determine what this figure should be, we have at present no other source of information than estimates of plant disease losses. However interesting it may be to work out theoretical controls, in the actual practice of plant pathology we have no right, outside the field of ornamentals, to recommend the use of any control measure which does not cost demonstrably less than the probable loss from the disease. On a very much larger scale, how is it possible to evaluate the results of campaigns for the eradication of long-established diseases, for instance, fire blight and peach yellows, unless or until we know something about the extent of the fluctuations in these diseases when there have been no eradication campaigns?[9]

The difference of opinion as to the value of crop loss estimates would seem to boil down to this: a difference of opinion between those whose first concern is an alleged mathematical accuracy and those who are seriously concerned with practical disease control and adjustment to disease conditions in a practical world. For my own part, I can see little justification for a plant pathology—certainly not for a plant pathology supported by public taxation—which is more concerned with methodology than with objectives.

I should like to go even further than this and insist that we are in serious

[8] N. E. Stevens and J. I. Wood, *Bot. Rev.*, 3: 277-306, Fig. 18, 1937.
[9] *Jour. Econ. Entom.*, 31: 39-44, 1938.

danger of needlessly limiting our interests and usefulness, to say nothing of making ourselves ridiculous, by insisting on professionally ignoring all phenomena which do not lend themselves readily to measurement by fashionable technics.

Fungus spores can be measured in fractions of microns and the geographic ranges of fungi only approximately determined, while the causes of their distribution can be little more than guessed at. Shall we, therefore, spend our lives measuring spores and neglect questions relating to distribution and its causes?

Because it is impossible to determine whether the loss caused was 18.2 per cent or 22.4 per cent must we refrain from recording the fact that *Diplodia zeae* caused severe losses in Illinois in 1938?

Shall we fearfully protect our precious reputations by refusing to publish any suggestions or opinions not bolstered with measurements which seem to be statistically respectable?

Through the indulgence of the Wisconsin Academy of Science, we have been able to place in print a conviction held by myself and two associates that attempting to utilize alkaline flooding water in the cultivation of cranberries in that state is apt to lead to financial disaster. Of this, as yet, we have not a shred of experimental evidence, nor is there any chance of getting that within the next few years. We know, however, that of the properties now under cultivation those with alkaline water are the hardest to manage successfully. We know that an entire cultural area, once the largest in the state, which has alkaline water, is now almost abandoned; while another with acid water is now cultivated by the third generation of successful cranberry growers. We know that the only property in the earlier area still under cultivation is in the red and has been in deep red for at least a quarter of a century. We have case history after case history, some running back 45 years.

In spite of all this evidence some of our colleagues (fortunately not the cranberry growers themselves) find it hard to take the conclusions seriously. If, in contrast to the above, we were reporting results obtained from six 4-inch pots for one year, our results would be far more respectable.

A realization of the possibility that even in science there is danger from too devout worship of the fetish of alleged accuracy has recently been found in papers from two fields which are commonly supposed to be much more characterized by accuracy than biology can reasonably hope to be; namely, astronomy and physics. Professor Henry Norris Russell, in his presidential address before the American Astronomical Association[10] in 1937, discussed "the place, utility, and limitations of approximate methods in astronomical work."

Professor Russell's paper is short enough to be easily read and too compact to be easily abstracted. He gives a number of instances in which approximate methods have given highly significant results, and raises the important point of what he calls "astronomical economics," that is, the question as to the extent to which the director of a great modern observatory should spend money and energy in securing more and more accurate observations. He indicated that spending effort and funds for accuracy is

[10] H. N. Russell, *Publications Am. Astron. Soc.*, 9: 108-114, 1938.

justified to the extent that the problem under study requires accurate measurements for its solution.

Even in astronomy it appears there are those who would let methods dictate problems. He quotes E. C. PICKERING as saying that shortly after he had become director of the Harvard Observatory he was severely and publicly criticized by a conservative group because "instead of putting his time on meridian observations, which can be made with an accuracy better than one part in a hundred thousand, he is working on photometry, with errors of ten per cent or worse, and in spectra, with no accuracy at all." And, he adds, "PICKERING had the courage of his convictions, and kept on with the results that we know."

CHARLES GALTON DARWIN in a discussion of "Logic and Probability in Physics"[11] says: "What is the moral of all this? It is that the new physics has definitely shown that nature has no sharp edges; and if there is a slight fuzziness inherent in absolutely all the facts of the world, then we must be wrong if we attempt to draw a picture in hard outline."

A drastic change in my surroundings a few years ago led to my reading a number of books in the field of sociology—which is after all a sort of biology. I find these workers are frequently faced with this same choice, relatively accurate measurements in a less interesting and significant field or obviously crude measurements in a highly interesting one. May I then close with quotations from two of them, "We have been choosing the problems of study not so much by their importance as by a possibility of making a 'fine and accurate study of a topic.' . . . Pushed too far in that direction, these investigations become a worthless parody on science. To avoid this situation, once in a while, somebody has to take upon himself the doubtful privilege of selecting an important topic for his study, though it does not lend itself to an exact investigation."[12] "Method must conform to material and not vice versa . . . the first loyalty of a scientist is to his material; . . ."[13]

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[11] *Science*, 88: 155-159, 1938.

[12] P. A. SOROKIN, "Social and Cultural Dynamics," Vol. 2, p. 270.

[13] JOHN DOLLARD, "Caste and Class in a Southern Town."

— XI —

HOW PLANT BREEDING PROGRAMS COMPLICATE PLANT DISEASE PROBLEMS*

The present interest in breeding crop plants, together with present means of wide and prompt dissemination of new varieties bids fair to still further complicate disease problems, whether of control, prediction or even of estimation. Given a fairly stable agriculture based on varieties long established in a region, serious fluctuations in the severity of a disease, the kind which are of great economic importance and incidentally of much scientific interest, usually were induced in one of two ways—a marked change in the weather for a longer or shorter period, or the appearance, by hybridization or mutation or by actual introduction, of a new parasite. So generally has this been the case that some pathologists have found an apparent relation between the age of an agriculture and the losses from plant disease. To these students it has seemed that plant disease losses were greater in the regions of newer agriculture.

The list of tragic losses in important economic plants due to introduced insect pests and plant diseases is a long one, too well known to need repetition here. Of the converse process, a serious outbreak of a disease or an insect pest following the introduction of a susceptible variety into the domain of a potentially serious parasite, the history of the potato beetle is perhaps the best known example.

If the historical accounts are correct, the potato beetle acquired its appetite for potato plants very soon after the introduction of the new host. The cornborer was apparently more deliberate and according to Wardle,[1] "We have historical evidence that the corn-borer thirty years ago was in southern Germany a pest of hemp and hops, and that since its adaptation to corn, it will not seriously attack hemp and hops, even when these plants are grown in close proximity to heavily infested corn fields." Of course, the slightly more than four centuries during which corn had then been cultivated in Germany is really a short time compared with the more than forty centuries which deCandolle believes hemp had been cultivated, but it is entirely too long to be regarded as a practicable test or quarantine period.

To these two well-recognized methods by which the incidence of losses due to disease and insect pests is strongly influenced, namely, weather

* First published in *Science* 95: 313-316 (1942).

[1] R. A. Wardle, "Problems of Applied Biology." 1929.

changes and introduction, there is now being added, I believe, a third, the work of the plant breeders. It may be worthwhile to consider future possibilities on the basis of past performance, the only method yet devised for any intelligent appraisal of the future.

On the basis of past performance we may reasonably expect interesting developments in the distribution of disease-producing organisms. As an example of this may be cited the case of the introduction into the northern states of the nematode which causes summer dwarf of strawberries. In the spring of 1930 strawberry plants of the then new and highly regarded variety, Blakemore, were shipped from a region where this type of strawberry dwarf was common[2] to many points outside its known range. Apparently no complete survey has been made since 1931, but the subsequent careful work of CHRISTIE[3] leaves little doubt that the disease then introduced into the eastern shore of Maryland is gradually becoming less abundant.

Now that breeding for disease resistance attracts so much attention, we must expect the introduction and rapid dissemination of varieties very resistant to certain diseases. Examples of this are easily recalled. Since they represent a distinct economic gain and disturb no one but the student of plant diseases, they get much attention elsewhere and require little here.

Two closely related results of breeding programs which seem worth special consideration are the introduction on a commercial scale of varieties very susceptible to certain, sometimes new, diseases and the modifying effect of new varieties on parasites long known to be of commercial importance.

At first sight, to speak of the introduction on a commercial scale of varieties very susceptible to certain diseases may seem like a criticism of our breeding programs, but it is not so intended. I struggled with this problem for several years, and became convinced that with our available knowledge, it simply is not possible to so fully test our new varieties as to offer assurance that some of them will not sooner or later prove very susceptible to some disease. This should not be interpreted as condoning any laxity in tests for disease resistance which, of course, should be conducted with all possible completeness and severity, but the number of potential pests is so great and the conditions under which they might become serious so varied that any all-inclusive test would take so long and require culture under so wide a range of environmental conditions as to be wholly impracticable. Just as the real test of a new model of automobile is the road test in the hands of the first twenty to fifty thousand owners, so the real test of a new variety is its culture in the hands of ten to twenty thousand farmers.

As evidence on this point, I would like to quote from the summary given by STAKMAN, CHRISTENSEN and BECKER[4] of the experience in the spring wheat area of the United States of America during the last 25 years. These writers say, "During the time of observation at least twenty new varieties of wheat were introduced into the spring wheat area and almost every one brought new disease problems with it, or at least changed the old

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[2] N. E. STEVENS and P. V. MOOK, *Jour. Econ. Entomology*, 25: 447-454, 1932.

[3] J. R. CHRISTIE, *Jour. Agric. Res.*, 57: 73-80, 1938 (and earlier papers therein cited).

[4] E. C. STAKMAN, J. J. CHRISTENSEN and HANNA BECKER, *Der Züchter*, 10: 57-68, 1938.

ones." As specific examples they point out that for many years Haynes, Glyndon and Preston were the wheat varieties commonly grown. Stem rust and bunt were then the only really important diseases. When Marquis replaced these varieties over a wide area bunt no longer played any role and stem rust seemed somewhat less serious, but scab, which earlier did not have any significance, suddenly became predominantly important, since the new variety was much more susceptible to this disease.

Somewhat later Hope and H-44, neither of which is in the opinion of these authors a very good variety, were introduced. These are in general more resistant to most of the harmful diseases of spring wheat than any other varieties now available, and are being extensively used for crossing. However, there appears to be a strong linkage in many of these crosses between rust resistance and susceptibility to black chaff to which Hope and H-44 are much more susceptible than any other spring wheats which have been observed up to that time. Just as the wide-spread planting of Marquis made scab of real economic importance, so the wide-spread planting of Hope and H-44 might make black chaff of real economic importance.

In the same paper these authors review the situation regarding barley and state that the disease problem in the case of this crop has changed repeatedly with the introduction and culture of new varieties. In discussing the varieties Velvet and Glabron, which were introduced in 1926, they say "but after these varieties were generally grown for several years it was observed that they were very susceptible to loose smut and scab. This latter disease is dangerous because it not only greatly lessens the yield in the case of an epidemic, but also makes the variety useless for feeding to swine because of its toxicity.

What may perhaps be regarded as the converse of the relation just described is the effect on parasite populations resulting from the introduction of new host varieties, varieties which by their popularity replace those previously grown and thus make inevitable the development of certain strains of these parasites previously so rare as to be insignificant or even unknown. Here again the work of STAKMAN and his associates furnishes a good example. Who can fail to connect the increase in form 56 from 0.2 per cent of the rust population in 1930 to 30 per cent of that population in 1934 with the coincident great increase in the planting of Ceres wheat, first released in 1926, to which variety stem rust proved so disastrous in 1935?

In considering future possibilities regarding plant diseases it would be folly to exclude what is in some ways the most significant and far-reaching change in a crop plant in our generation, the very general planting of "hybrid" corn. Unless you live in the corn belt it is difficult to comprehend the speed and extent of the spread of the vogue of hybrid corn. According to Dr. MCCALL's report to the chief of the Bureau of Plant Industry, 1939, the approximate acreages of hybrid corn were 500,000 in 1935, 1,500,000 in 1936, 3,500,000 in 1937 and 17,000,000 in 1938. In 1939 at least 25,000,000 acres of corn in the corn belt were planted to hybrid seed.[5]

It seems hardly possible that so profound a change can be made without altering the disease relations of the crop. This replacing of open-pollinated corn with hybrid corn fits into the problem we have under consideration in at least three ways. First, in the matter of introducing new varieties very susceptible to certain diseases. If any one imagines that the new

[5] H. A. WALLACE, *New Republic,* November 8, 1939.

varieties, or at least new numbers, of hybrid corn introduced into the corn belt during 1939 and 1940 were so thoroughly tested that their disease relations could be predicted, his knowledge of human nature is even more limited than his knowledge of corn diseases. Nor need we wait for the distant future or for the action of unknown diseases on untested hybrids for evidence of extreme susceptibility. Witness, for example, the havoc worked in Hybrid 960 during 1938 by bacterial wilt and Diplodia stalk rot.

Two of the most experienced entomologists in the Middle West assure me that a somewhat similar condition has been noted with relation to at least one insect. According to these observers, the corn leaf aphid, *Aphis maidis* Fitch, was of relatively minor economic importance for many years prior to the introduction of hybrid corn. During recent years, however, with the introduction of a number of very susceptible hybrids the importance of this insect has greatly increased.

What about the effect of hybrid corn on corn parasites? The change in wheat rust in response to the change in the host population was made possible by the variation continually going on in the stem rust organism itself. Corn parasites vary too, some of them, no doubt, even more actively than many wheat parasites. That the organism which causes bacterial wilt of corn is made up of a large number of strains, among which there is definite selection on repeated passage through a resistant host, has been proved by McNew[6] and by Lincoln.[7] McNew has further shown that slightly virulent cultures become more virulent after they have grown on certain media.[8]

Just how different the host environment of the wilt organism is in the northeastern sweet-corn-growing states from its environment in the same region ten years ago can be understood only if we review the history of Golden Cross Bantam. This is a single cross made up of two inbred lines, Purdue 39 and Purdue 51. In 1931 seed of this extraordinary hybrid, developed by Glenn M. Smith, was distributed to 61 interested growers in 20 states. Its progress since that time may be best traced from the details in this table.

Year	Golden Cross Bantam seed produced in pounds	Yellow corn canned: Open pollinated Per cent	Yellow corn canned: Hybrid Per cent	Proportion of all sweet corn hybrids used for canning which were Golden Cross Bantam Per cent
1932	2,000	..	..	..
1933	350,000	69	31	..
1934	600,000	57	43	67
1935	1,250,000	38	62	73
1936	1,500,000	27	73	72
1937	1,500,000	21	79	70

These figures were taken from reports of the Raw Products Bureau of the National Canners Association. Accurate figures for subsequent

[6] G. L. McNew, *Phytopath.*, 27: 1161-1170, 1937.
[7] R. E. Lincoln, *Science*, 89: 159-160, 1939.
[8] G. L. McNew, *Phytopath.*, 28: 769-787, 1938.

years are not available and may never be since there is a suspicion that Golden Cross Bantam is now being produced and sold under other names.

Not less remarkable than the rapid increase in total acreage is the wide range throughout which Golden Cross Bantam proves to be adapted. It is now in successful large-scale commercial production from Illinois east to and including New York and south through Maryland. Indeed in Maryland it makes up about 40 per cent of the total crop of sweet corn for canning, although open pollinated Golden Bantam was given up there prior to 1930 because of bacterial wilt. Golden Cross Bantam is now being grown successfully on a commercial scale in Oregon and Eastern Washington and in Western Idaho, though that is not important in the present relation.

The important point here is that from Illinois to the eastern seaboard, throughout practically all the region in which bacterial wilt was so destructive on sweet corn in 1931-1933, its host environment, so far as sweet corn is concerned, has become quite different. Instead of open-pollinated corn, so variable as to become locally adapted and not profitably planted far from the point of production, we have a very uniform single cross dominating sweet corn production over at least a thousand mile belt. Given a parasite which we know to be highly plastic, can any reasonable person assume that the population of this organism today can be the same as in 1931?

At some risk of seeming to attempt the dramatic I would like to emphasize the fact that the past decade is the first in the history of agriculture when one could speak of the modification of a corn parasite by a uniform host environment on a scale large enough to be readily observed, and expect to be taken seriously. I believe it may be literally true that never before in the history of corn culture has a corn parasite been exposed over a wide range to so uniform a host environment. Biologically, including of course its disease relations, we are transforming Indian corn from a freely cross-pollinated crop to a synthetic product.

Elsewhere I have pointed out a relation[9] which may indeed have been all but self-evident, namely, that among comparable crops there is a demonstrable difference as regards disease losses between open-pollinated crops produced from seed and similar self-pollinated crops, disease losses fluctuating more and being in general of greater importance in those plants which are largely self-pollinating.

In a field of open-pollinated corn, there is, I am told, even in the named varieties a wide range in genetic composition and, no doubt, also variation in susceptibility to many diseases. In such a field there is an observable variation in the time of silking and tasseling, and in the time at which plants reach a stage of susceptibility to infection by certain diseases. In contrast to this a field of "hybrid corn" is much more uniform in these respects.

To be sure, the corn now planted consists mostly of double crosses which are somewhat more variable in time of silking, etc., than single crosses. Certain careful growers also plant two crosses in a single field in order that they may still further reduce the hazards due to too great uniformity.

More important in relation to disease and disease-producing organisms is the fact that up until the last few years Indian corn apparently has been

[9] N. E. Stevens, *Science*, 89: 339-340, 1939.

able to vary and adjust itself about as rapidly as the disease organisms themselves. Recent researches have shown that in general the common smut of corn, like most smut fungi, is parasitic only in the dicaryon stage, that is, it must cross to parasitize; that it is extremely heterozygous for many characters and consequently unstable; that variation is so common, even in unisexual lines, that from a single monosporidial line more than 150 distinct variants have been isolated; that different combinations of monosporidial lines differ greatly in their degree of pathogenicity and that some of them are so virulent as to kill the host outright.

No one supposes that this ability of the smut fungus or other fungi is recently acquired. It is very much more likely that the process has been going on for many centuries. It is hardly to be doubted that exceedingly virulent lines of smut may occasionally have appeared during these centuries, yet Indian corn survived and has been grown during recent years with a minimum of effective disease control, and apparently much less loss from disease than in the case of wheat.[10]

In corn we are dealing with a native plant, or at least one long cultivated in this area. In many of the places where we now grow corn, our Indian predecessors grew the same plant, which held its own against these same diseases when our ancestors were suffering from the black plague or arguing about the Post-Columbian pandemic of syphilis. All that is now changed and I believe permanently changed. Hybrid corn is here to stay. The increased yield, together with the ease with which seed is produced, makes its future all but certain. Almost equally certain seems to me the probability that we are changing the disease relations of this great crop.

By the wide use of hybrid corn we are depriving this important crop of its power of taking care of itself and by continued crossing and variation, continually adjusting itself to the equally variable parasites which attack it, and have substituted our own choice of parents for each field. In view of our none too brilliant record of success in so testing varieties of other grains as to insure them against loss from disease when planted over a wide acreage, it seems unlikely that we will have much greater success in dealing with corn. It seems much more probable that in spite of the best efforts of the breeders we will see for some years in hybrid corn a disease relation more nearly resembling that in wheat; that is, wider fluctuations in losses with some years very little damage and in others greater damage than has heretofore been observed. Eventually we may see lower average disease losses, but I sincerely believe that our corn breeders will be doing all that can reasonably be expected of them, if during the next ten years they keep the losses from disease in the new varieties, including losses in decay after harvesting, down to the general level of such losses in the old open-pollinated varieties.

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[10] N. E. Stevens, *Scientific Monthly,* 52: 364-366, 1941.

— XII —

CAN THE EDUCATOR BE RE-EDUCATED?*

A Consideration of the Importance of Subject-Matter

AFTER twenty-three years, during which my chief contacts with universities were not always too successful attempts to utilize their products, in 1936 I returned to teaching. Because of my long absence from teaching, refamiliarizing myself included the "auditing" of a number of elementary courses in varied fields and some reading of and listening to professional educationists. In passing on some of the results of my observations, I ask the reader to remember that I write subject to correction, for this is certainly not my field of specialization.

Among other things, it appears that at least some of the more progressive educational psychologists are renewing their faith in transfer of training, and now again believe in the utility of what used to be called mental discipline. If I remember correctly, the possibility of both used to be categorically denied thirty years ago. To be sure, this present faith is not so all-inclusive as its predecessor. Rather definite limits are indicated for ready transfer. Nevertheless there seems to be something very close to a direct reversal of opinion on this all-important question.

One hears much now of "function" and "functional approach"—terms that seem to involve somewhat varied concepts. Some insist that a main purpose of education is to train the mind to function more effectively, and that education is concerned with mental functions, acts, or abilities. One finds, also, a frank—even an enthusiastic—admission that education attempts to contribute to the student's preparation for the situations and activities of everyday life. Another major desideratum is to provide for the integration of human knowledge and experience; that is, to establish for the student the essential unity of all learning.

All this sounds like exceedingly good news to teachers of science. The teacher of an applied phase of science is frankly engaged in attempting to contribute to the student's preparation for life, not merely as a specialist but as an intelligent citizen. In the practice of almost any phase of applied science it is impossible to forget the essential unity of all learning—indeed, of all human effort. In an applied field there is possible no other approach than the "functional," if I understand the term correctly.

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* Invitation paper before American Phytopathological Society, June 26, 1942, Toledo, Ohio.—First published in *Journal of Higher Education* 14: 75-78 (1943).

Finally, unless transfer of training actually and frequently occurs, teachers of applied science are certainly wasting a good share of their time. No one knows better than they do that the problems which they are now discussing may disappear or become unimportant, and that some of the solutions, now the best they have to offer, will soon prove inadequate. WHETZEL, of Cornell, is quoted as saying to successive classes in Elementary Plant Pathology, "90 per cent of what I tell you may be wrong." Whether he ever said it or not, it is largely true and sounds like him. Unless the men being trained now can use that training in finding new solutions for new problems, they are less capable and less well trained than their predecessors.

Certainly, on the basis of these common aims, we might well expect from the educationists wholehearted endorsement of integrated, specialized courses in applied fields of science as the most suitable means of attaining the highly desirable ends they have listed. On the contrary, as tools useful in attaining these ends, many of the educationists discard, indeed specifically exclude, technical, vocational, and professional training, all specialized education and concentration in narrow fields of knowledge, and would substitute therefor what are called "general" courses. I am not at all sure that this is a wise choice, and I am becoming increasingly fearful that we teachers of special subjects may be largely to blame for it.

One major reason alleged for the rejection of material belonging to the classes just indicated appears to be the fear that zealous teachers of such subjects may become interested in the subject-matter to such a degree that they will give it too much attention and the student too little. Undoubtedly we have enthused a good deal over the importance of the subjects we teach, over their intrinsic worth, their utility, and their absorbing interest. But all the time we knew we were teaching students; and we assumed the educationists knew it also. Obviously, the student should come first in our attention; obviously, also, the less one teaches the less danger there is of becoming really interested in the subject, but there must be wiser ways of assuring first place to the student than diluting or diffusing the material taught.

A second reason for the educationists' rejection of courses with readily unified subject-matter seems to be the conviction that these units are artificial and do not represent "problem areas." It is suggested that our present separation of the sciences is a recent and artificial thing, something developed in the nineteenth century in response to some concept of the structure of the universe, or of a system of the sciences. We and our immediate predecessors may well be to blame for this. We have put a lot of emphasis on what we called "classification" of sciences.

I was taught and may have believed that biology is a science of life and includes numerous divisions, such as, botany, physiology, and zoology. Historically, of course, the exact opposite seems to have been the case. One of man's basic problems through the ages has been how to get along with and get the most out of plants. This is a very old and very important problem area. It may be literally true that to the angiosperm seed more than to any other structure the economic evolution of the human race is due. From this struggle and within this problem area developed what we call the science of botany. Meanwhile, man was slowly learning how to get along with animals, which he could scarcely have conceived of as related

to plants; and no doubt he soon became conscious, probably uncomfortably conscious, of smaller moving things that seemed to him neither plants nor animals.

It is interesting to note that neither the word *biology* nor the concept of biology as a science of life appeared until 1802; we in science remember that it was not until 1861 that MAX SCHULTZE first recognized the identity of animal "sarcode" and vegetable "protoplasm." I am convinced now that the reason that courses in biology have so often failed is the same that makes biological societies and unions of biological societies so hard to keep alive. Namely, that they represent artificial syntheses with no natural foundation in our minds and lives. Yet a synthesis of botany, entomology, physiology, and zoology really should be easy compared with what some of our associates are asked to undertake.

During the year just past I have watched at close range while four able men tried to produce something teachable with material drawn from astronomy, chemistry, geology, and physics. The results were interesting and instructive, but my own conclusion is that what they were undertaking was not much easier than would have been an attempt to teach simultaneously the rudiments of French, German, Italian, and Spanish.[1] Of course, all science like all knowledge has a fundamental unity, but no one, at least outside institutions for the insane, tries to think of it all at once. Each of us thinks of a special field of interest and then relates it to other fields at as many points as his particular mind can grasp.

What, the reader may fairly ask, has all this to do with my topic, with the sometimes highly specialized, technical, often useful, and always absorbingly interesting subject-matter which most science teachers are paid to teach? Merely this: I am rapidly reaching the conclusion, by no means new, though it is new to me, that in just such subject-matter the educators of the future may well find the real material for their functional approach, for training the minds of their students and acquainting them with some of the essentials of the integration of human knowledge and experience.

Of course I might just go ahead and assert this as something of which I was entirely convinced That seems to be one effective way of getting opinions taken seriously. On the other hand, my training leads me to lean rather toward the experimental method. If we or any of us could find an open-minded educational psychologist with some time and a little money, I believe a series of experiments could be set up to measure the actual utility as educational material of so specialized a field as economic entomology, for example, as compared with what is called biology.

After this time and money had been spent we might find, as all of us have found so many times in our own work, that the experiments had not been well planned, or that we could not interpret our results. Or we might find that these specialized fields were not particularly suitable as material for general education. On the other hand, my observations during the past three years have led me to believe that there is a fair chance, a whole lot better chance than some of our own experiments have of success, that our experimenters might conclude that they had been rejecting the very best material for their educational purposes. If so it would not be the first

[1] This sentence was written with the thought that it expressed the height of the ridiculous. I have since learned that just such courses have been attempted.

time in recorded history that the stone which the builders at first rejected became finally the head of the corner.

After all, few of us are MARK HOPKINSES, and our classes are not made up exclusively of BACONS, DARWINS, and NEWTONS. At least the subject-matter of our courses is teachable, is unified and comprehensible within a reasonable time by fairly good minds. We are, of course, not merely teachers of plant pathology, but students, as well. Probably this very fact leads us inevitably to question seriously whether it is possible for a man to teach a subject that he cannot even study.

— XIII —

APPLIED BOTANY AS FUN*

MUCH ink has been spilled over the division (real or imaginary) of "pure" from applied science.

The thesis which I should like to develop—confining myself to botany, the only field in which I can claim authority—is that the difference between the fields is real but that it lies—not in the methods which are largely identical, not in the basic scientific values which are much the same, not even in volume of practical results which may flow from the seemingly least practical approaches—but in the method of choosing problems; that the difference between pure and applied botany is one not of content but of intent. If you choose your own problem (or think you do) it is pure botany. If your problem is chosen for you or forced upon you by the economic needs of some group, it is applied botany.

I shall contend further that the advantage in some respects lies on the side of applied botany—specifically that if you happen to be made that way, it is more fun.

First of all, applied botany offers variety. I have never made any claims of purposefully pushing outward the boundaries of knowledge, rather have I been pushed around by the exigencies of the period. Yet in my 25 years spent in the service of economic botany, I had few dull periods. I have been associated in the field for longer or shorter times with four of our great outbreaks of plant disease, the chestnut-bark disease (the greatest of all), bacterial wilt of corn, downy mildew of tobacco, and the Dutch elm disease.

Geographically as well as botanically, my interests have been forced to range widely. Fortunately, plants grow at the most pleasant seasons. They can be studied in Alaska only in the summer and may well be studied during the winter in Florida or Hawaii. I have, however, found it necessary to work at the South Water Market in Chicago and on the water front of New York at three a. m. in the dead of winter. During studies of spoilage of fruit in transit I spent some days in a superintendent's private car and a good part of one night on top of a freight car crossing Iowa. I have even doubled for five summers as an entomologist, and believe it or not, even entomology is interesting if properly pursued.

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* First published in *Transactions of the Illinois State Academy of Science* 36: 105-106 (1943).

On the whole, economic botany tends to be a sociable job. In our more or less futile attempts to control plant diseases during the past 25 years, we have taken some bad beatings, but our company has been numerous and exceedingly good.

I very much hope that I will not be misunderstood when I say that I rate as one of the real advantages of economic botany the opportunity, indeed the compulsion, to take real chances on things that really mean something. I bet but rarely, partly, no doubt, through parsimony, but largely as a result of a conviction that if a thing is not of sufficient interest without a wager, it is not worth bothering with. I note in a book review in the *Yale Review* for 1938 a sentence referring to the "arid tradition of American culture which compels professors to be accurate and sound rather than interesting and speculative." In the economic field you simply have to take a chance—to act, or refuse to act, which is the same in the end—to pit your own wit against nature and forecast the outcome.

Another of the real advantages of economic botany is that it is perfectly respectable to use plain understandable English. A few years ago I came across (in the December, 1936 *Atlantic*) an article entitled "The Snobbishness of the Learned" in which this statement was made:

> "The impression that philosophical and scientific ideas cannot be explained in plain language to plain people is . . . due to the fact that philosophers and men of science have not, as a rule, the wit to do it. It is due in plain terms to the stupidity of the learned men and not to the stupidity of humanity."

I believe the charge is unfair, at least I have a strong suspicion that the chief reason so many of our colleagues do not use plain English is that they do not dare, or at least they feel they appear more profound if they are obscure. This is well expressed—as are so many other important truths—by GILBERT and SULLIVAN. This is from "Patience."

> "If this deep young man expresses himself in terms too deep for *me*,
> Why, what a very singularly deep young man, this deep young man must be."

Of course, sometimes scientific writers go too far even for their associates. Some of you may have read a recent book review, which may or may not have been justified. (I have never read the original.) It begins:

> "This book is a fine example of an important and already difficult subject discussed in an abstruse, involved, pompous, and thoroughly tiresome manner. Simple things are made complex, and complex things are made well-nigh incomprehensible."[1]

Finally, economic botany seems to me to be fun because it has human interest. HANS ZINSSER refers to this in his recent autobiography, the title of which is "As I Remember Him."

> "The scientist's temporary relief from constant dissatisfaction with his own accomplishments comes from those interludes in which he projects his technical and theoretical training into a problem of practical application."

MARY E. PENNINGTON expresses the same idea in an interview published in the November 10, 1940 number of "Chemical News."

> "There is a thrill when a scientific idea suddenly strikes home in the form of an industrial problem."

I suppose it all boils down to the universal human need of being important to someone. We all recognize this need in other people and oc-

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[1] *Copeia*, Dec. 26, 1939, p. 240.

casionally in ourselves. That is why some people keep dogs, why ladies no longer too young drop their handkerchiefs where we have to pick them up, why neurotic wives throw sick spells. That incidentally, is why honorary societies are organized and perpetuated. Apparently such devices are necessary to sustain the enthusiasm of the scientists themselves. In the economic field there is little or at least much less, need for such secondary stimuli. Indeed, the danger is quite the other way. Scientific work is being held in such high esteem these days that the grave danger is not that we will not be listened to, but that our advice will be taken too seriously and applied with too little restraint.

We can cease to worry about priority in publication when our ideas become common knowledge in an intelligent group almost before they could be published, and in view of the very large percentage of mistakes that each of us is sure to make, it is worth something to be in a field where even mistakes are important.

— XIV —

IS TEACHING ABILITY RECOGNIZED?*

LIKE a refrain one hears in current discussions of academic problems remarks like the following: "These objectives can be obtained only if the teaching ability of faculty members is given as much recognition as is given to research ability." "Teaching ability is not rewarded by our colleges as is research ability."

If some one does not soon question the accuracy of these statements they will come to be believed through mere repetition. The first time I ever heard the validity of such assertions openly and adequately challenged was during the meeting of the American Society of Agronomy at St. Louis in November, 1942. A session was being held on teaching and its problems. A guest speaker had repeated the time-worn remark that in our colleges teaching is not rewarded as is research. In the course of the discussion which followed Dr. H. K. HAYES, of Minnesota, offered the comment that teaching ability in that field was recognized and rewarded. He added that if necessary he could present the proof.

The discussion went on. As I was a visitor, only a few of the men present were known to me personally. It was, however, evident from the remarks that many of them were men of unquestioned eminence in their field. The group evidently included a good number of heads of large departments of agronomy and a sprinkling of deans of agriculture. Finally some one asked Professor HAYES for his proof. His reply, which I quote from memory, was somewhat as follows: "I have objective proof. It is here in this room. I do not wish to embarass anyone so I will not name individuals unless someone insists, but I see here a goodly number of individuals of recognized standing and influence in their fields whose positions rest on their recognized ability as teachers rather than as investigators." That ended the discussion.

One result of the discussion thus ended was that I started a survey of the teaching of botany in the United States during the past generation. Some portion of the material assembled will be published elsewhere. One of the conclusions to which I have come is a wholehearted agreement with Professor HAYES's spontaneous outburst at St. Louis. It makes little difference what objective criterion of eminence one chooses provided the list contains a fair number of names. A list of presidents of the Botanical

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* First published in *Science* 99: 101 (1944).

Society of America will serve or a list of the presidents of any of the other societies concerned with plant science or the chairmen of Section G, or of those who have received the now much discussed "stars" in "American Men of Science." In any case one finds a large percentage of those who are known first and foremost as teachers. This is particularly impressive when it is realized how many of our colleagues have to give all their time to research or administration.

The same thing may not be true in fields other than those of the plant sciences. At least the question may fairly be raised regarding them. Of course I have no information as to the salaries received by these outstanding teachers; that seems to be the critical point, but it seems unlikely that they have been conspicuously less well paid than their fellows.

Apparently one source of the assertion so freely made that teaching ability as such is not adequately rewarded is the failure of those who make it to recognize that teaching ability may be coupled with other abilities. In other words, the mere fact that a member of a college faculty is unable or unwilling to carry out a research program does not constitute *prima facie* evidence of teaching ability of a high order.

— XV —

THE ANECDOTE AS AN ANTIDOTE TO STATISTICAL ANALYSIS*

WHETHER or not you agree with my scientific thesis, which I insist is of real importance, I feel sure that I can introduce to you one of the most fascinating of intellectual avocations. Most students of the life sciences have an urge to collect something. My collection consists of material which does not have to be dried or pressed, pickled or dusted, protected from molds, insects or breakage. It is a collection of coincidences. The importance of this collection will, I hope, soon appear.

I should like to try to reestablish in your minds the realization that the *improbable* is still *possible;* that rare events do indeed happen; that a verified fact is of more importance in human living, as in science, than the proof that an event is *mathematically probable.* This seems to me well worth your time and mine because the contrary concept is now so popular that it has greatly influenced the interests and methods of botanists—if not all students of the life sciences in at least three ways.

They now largely refuse to study or even to consider what cannot be measured with conventional degrees of accuracy. They have thus largely abandoned field work for the much more easily interpreted laboratory work.

In experimentation they have become too ready to accept as true whatever appears statistically significant.

They have in too many cases acted as though what is statistically improbable is, as a matter of fact, *impossible*—that is it simply could not happen. This type of thinking enters most seriously into discussions of the means of evolution. The difficulty is not mathematical, it is psychological.

Many thoughtful students of the life sciences find it hard to believe that devices and responses so intricate as are frequently found could have arisen as a result of fluctuating variations or even through mutations. This, I believe, accounts for much of the interest in such theses as creative evolution. The case of the Lamarckians and the Neo-Lamarckians rests largely on this basis; namely the assumption that if an event was sufficiently improbable, it just could not have happened at the time it was needed in order to account for the development of some particular plant and animal organ or response. Many people simply cannot believe that a thing really happened if it was mathematically improbable. This attitude I would like to

* Not previously published.

combat by the simplest of all methods; namely, reminding you that unusual events do occur.

Curiously enough the prejudice (for I believe it to be just that) against admitting the possibility of the improbable runs through all printed matter. In the words of KENNETH ROBERTS (*Trending Into Maine,* page 22) "Respectable novelists are loath to permit coincidences to appear in their stories for they're difficult to believe, and they reflect on the inventiveness of the author." This attitude is reflected, of course, in the proverb, "Truth is stranger than fiction."

According to ARCHIBALD MCLEISH ("Public Speech in Poetry," page 545, *Yale Review,* Spring 1938), "History is supposed, by the economic determinists, to proceed without the cheap and vulgar aid of coincidence." Yet much of the interest of reading history lies in these very coincidences. You all recognize the motto: "Proclaim liberty throughout all the land unto all the inhabitants thereof." It is, as you learned in the second grade, a quotation from LEVITICUS 25:10 on the Liberty Bell in Philadelphia, which was rung on July 4, 1776. Yet the bell was cast in England in 1751, and recast here in 1753. Or take another familiar example connected with the same great day. The sub-committee appointed to draft the United States Declaration of Independence consisted of three radical leaders. One, FRANKLIN was already old. The others were much younger. JEFFERSON, who wrote most of the document, was thirty-seven and JOHN ADAMS, who chiefly defended it in the debate, was eight years older. What then were the actuarial chances that these two men would die the same year? What were the chances that they would die the same day of the year, or that the date of their deaths would be an anniversary of the date the Declaration of Independence was signed, or that it would be the 50th Anniversary? Yet both died on July 4, 1826.

Every observing person of experience admits the occurrence of extraordinary coincidences in his own life. Yet fiction, human history, and biological evolution are supposed to proceed without them. My own attention was directed to this line of thought by the following incident. In the summer of 1932, I was driving through northern New York on official business in a government truck. The truck was nearly out of gas, and I was nearly out of money. I had, however, a credit card on the Shell Oil Company, good at dealer agencies. Dealer agencies, however, seemed to be far apart, so I stopped in the town of East Aurora at a small grocery store which had a Shell Oil tank, and asked the proprietor if he could give me credit. With perfect courtesy and obvious regret, he said that his was not a "Dealer agency" and he was not authorized to sell gas on a credit card.

My situation was getting serious, and I have no doubt that I spoke with earnestness and conviction. At any rate, a man in a coupe which bore no mark to indicate its official connection called the proprietor over to him and asked him what I wanted. Then he called me over and I stated my case. The stranger in the coupe said, "fill up his tank and I'll sign the slip." He was the assistant sales manager for the Shell Oil Company in that district. Anybody with mathematical inclinations might be interested in calculating the probability of the assistant sales manager making his inspection on that particular day and visiting that particular station during the five-minute period I was there trying to get gas on credit.

During the years since 1933 I have piled up a very considerable list of

extraordinary but provable coincidences. This includes a number of unusual events which resulted in real and observable benefits. There were also many for which no possible utility could be imagined such as an incident which was widely published in 1942. In Washington, D. C., in the George Washington Hospital, on August 13, of that year, between 4 and 5 p.m. two sets of twins were born. One set were the children of Mr. and Mrs. RAYMOND MIKESELL; the other children of Mr. and Mrs. ALFRED H. MIKESELL. The parents were not related and had never seen each other before. The name MIKESELL is rare. It occurs only five times in the current (1945) Chicago Telephone Directory as compared to 3,798 Andersons and 3,503 Smiths. Moreover twin births are comparatively rare. For the United States as a whole, twin births are about 1 in 100 of total births; and identical twins, which these clearly are, constitutes about one third of the total twin births. You might like to compute the probability of identical twins with parents of a name so rare as MIKESELL being born within a few minutes of each other in the same hospital.

In presenting this material before seminars at George Washington University, Louisiana State University, the University of Oklahoma and elsewhere, I have usually included at least a dozen examples. It always developed, however, that a considerable portion of the audience became greatly interested in supplying similar instances from personal experience.

Advantage is here taken of this tendency to invite possible readers to fill in as many personal recollections as they choose.[1]

The danger in this lies of course in the fact that interest may be concentrated on the anecdotes themselves rather than on the main thesis; which is, that in all our thinking and in all our experimentation, to say nothing of our teaching, we must not forget that rare events, even mathematically improbable events, do happen.

If some are tempted to regard this method of presentation as unscientific, I beg leave to add the following definition of the scientific method taken from the recently (1945) published report of the Harvard Committee on "General Education in a Free Society":

> "In reality the scientific method, of which so much is spoken for both good and ill, is whatever means may be appropriate for solving problems in the natural environment. The working scientist brings to bear upon these problems everything at his command—previous knowledge, intuition, trial and error, imagination, formal logic, and mathematics—and these may appear in almost any order in the course of working through a problem.
>
> "The nub of the matter is that the problem be solved."

Some other recent writer, in a paper I have not been able to locate so I cannot give him credit, paraphrases this somewhat more briefly:

> "The scientific method is merely doing ones damndest with ones mind on the present problem."

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[1] Numerous examples of coincidences which have proved significant in the progress of sciences are given in:

(1) FERNALD, M. L. Early botanists of the American Philosophical Society. Proceedings, Sept. 25, 1942.

(2) CANNON, W. B. The role of chance in discovery. Sci. Mo. 50: 204-209, 1940.

(3) A series of excellent coincidences all of which relate to the meeting of long separated one-egg twins are given in "Multiple Human Births" by HORATIO H. NEWMAN: pages 177, 179, 185, 187.

— XVI —

OBJECTIVES IN BIOLOGICAL COURSES*

THIS program was arranged by someone who understood both the problem and the English language. In this title the nouns are in the plural which clearly indicates the assumption that there are many types of courses in the biological sciences and that any one of these courses may properly have several objectives. Varied as these courses are, most of them fall readily in one or another of three groups.

Advanced Courses:— In the category of advanced courses are included only those which may contain some students who hope to have a part in what President LOWELL[1] indicated as the greatest function of higher education, namely, the increasing of knowledge. In such courses the objectives are obviously three, or if you prefer, five.

To indicate the knowledge already available in the given field and the means of keeping abreast of that knowledge.

To indicate the great gaps in that knowledge and perhaps suggest those which seem most likely of being filled with the means at hand.

Finally to insist that the student be able to make clear to other people what he has learned; that he be able to use graphs, tables, equations, symbols, and good plain English, written and spoken.

No apology is made for including this as one of the major objectives of advanced courses in any of our fields. All our teaching, including our examination techniques should be arranged to include this end. No piece of research is complete until its results are made available. It is not really complete until it is made really intelligible. Many of the classics in the field of the life sciences are great pieces of exposition. Have you read one of DARWIN's books lately, or MENDEL's great paper either in the original or in translation? (By contrast you might read any of STRASBURGER's later papers.) Of course, not many of us can reach these levels, but that is no excuse for not trying or for not insisting that our students try.

Most workers in the life sciences apparently agree that good exposition is an important part of mastery of our fields. HERMAN VON SCHRENK was, as you know, one of the earliest plant pathologists to enter the con-

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* Delivered before Section Q of the American Association for the Advancement of Science, Cleveland, Sept. 14, 1944.—First published in *School Science and Mathematics* 46: 551-559 (1946).

[1] LOWELL, A. LAWRENCE. "Some Functions of Higher Education," The Yale Review, Autumn, 1941.

sulting field. A couple of years ago, he closed a talk of absorbing interest to undergraduates of our College of Engineering with a plea to the boys to learn to write and speak the English language.

It is by no means suggested that workers in the biological sciences are alone in recognizing the importance of verbal expression. On our own campus the strongest support for adequate instruction in English rhetoric has come, I am informed, from the chemists. During recent years they have been vigorous supporters of adequate instruction in other modern languages.

Service Courses:— The phrase "service courses" is used in an endeavor to dodge "propaedeutic" which sounds too much like "pedantic." The term used has the added advantage of making it clear that consideration is not being given to courses intended primarily to start specialists in our own fields. It is extremely doubtful if any such beginning courses, in Botany at least, exist in the United States. On this point the report[2] of the Committee of the Botanical Society of America on the Teaching of Botany has some pertinent information. Significant of the attitude of American teachers of botany are their judgments as to the importance of various objectives in the general course. Out of 250 replies to a questionnaire on this subject, "interest in pursuing plant sciences as a profession" received only 23 votes and stood 56th in the list of 59 objectives considered.

Here are included all the courses regularly required in established pre-professional or undergraduate professional curricula; bacteriology for pre-medical and home economics; botany and entomology for pre-forestry and agriculture; physiology for physical education and home economics; finally, and perhaps especially, zoology for the pre-medical students. Students in such courses seem to be almost literally the forgotten men of current discussions of teaching in the biological sciences. During the past five years I have read rather extensively on this subject. Almost never are these required courses mentioned, except incidentally by educationists who assume that they are undesirable for general education. The very fact that such courses receive so little discussion makes me all the more skeptical of their up-to-dateness.

Pre-medical zoology must be among the oldest and most widely taught of these fields. There must be very few liberal arts colleges in the U. S. A., that do not offer some sort of pre-medical courses including zoology under some name. Perhaps the condition which apparently exists here is indicative of those which may develop in other courses similarly required over a long period. No accurate figures seem to be available, but the impression among my colleagues is that approximately three-fourths of the undergraduates taking zoology are doing so as pre-medicals. I am assured by them that undergraduate instruction in zoology is largely dominated by the supposed needs of this group. Some of the franker of them add "That is what is the matter with the teaching of zoology." By this they apparently mean that zoology courses have been so long and so generally cast in a single mold that great difficulty is experienced in changing them. This, I am told, is being discovered by a considerable number of college teachers of zoology who are now trying to make changes.

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[2] STOVER, E. L. *et al.* An exploratory student of the teaching of Botany in the Colleges and universities of the U. S. Botanical Society of America, 1938.

Whether this is literally true or not these "service courses" seem to be the ones most in need of restudy and the opinion is being forced upon me that they are the ones which most deserve the efforts of the very best teachers.

Here, in all seriousness, is our golden opportunity; a field ripe for the harvest. Here is our chance, in my opinion our best chance, possibly our only real chance, to do what our educationist friends urge us to do "to contribute,[3] at an appropriate level, to the student's preparation for the needs of everyday life . . . not only to the needs which a student sees but also those which he should be made to see—not only to life as it is actually lived, but also as it might better be lived—not to some narrow and highly specialized segment of life, but to life in its manifold aspects."

Incidentally, it must be next to impossible for any one to teach a subject in which he is interested to students in whom he is interested without trying to do all these things. Something might be gained, however, by making it perfectly clear to outsiders that we are trying to do them.

Here is a group of students to some degree selected, who must take our courses, who may be professionally active in related fields, who, if they make the grade, will have all too little opportunity for general study and, if they do not, will still become educated citizens. They are already aware of the specific professional usefulness of this work. Many of them, however, need to have pointed out to them the broader human implications and importance of what they are studying. Above all, students in such curricula are to a certain extent motivated. At least those who are in the curricula on their own volition rather than because of parental pressure are motivated.

This very motivation presents difficulties. I am assured that the home economics and pre-medical students of today, like the short course students at Kansas State College in 1912, tend to be skeptical of anything not obviously practical. This attitude, however, merely increases our obligation to see to it that the broader implications are insisted upon and driven home. If this wider view, these relations to general living are not acquired here, they are unlikely to be acquired later. With full recognition of the difficulties presented by too narrow motivation, it is my opinion that any motivation, even a materialistic or distorted or limited one, is vastly better than none.

What a difference motivation makes can be really appreciated only if you have to run the whole gamut. I honestly believe that I have, and if you will pardon a frank personal statement, it will illustrate what I mean. My best teaching during the past thirty years has been in the field, standing on the soil a man owns, among the plants upon which he depends for his living. Under such circumstances, the difference between *p*H and methyl orange alkalinity, or the critical difference in the levels of control needed in dealing with the insect vector of a virus and an insect which destroys a crop directly, or Blackman's theory of limiting factors become surprisingly easy of comprehension.

Last year, I had the privilege of teaching general biology in our Division of General Studies. It was a chastening experience—an experience which all of you should have who think you have ability as teachers.

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[3] Pothoff, E. F., "Fundamental Purposes of General Education," *Journal of Higher Education* 13, 73-77 (1942).

During the past thirty-eight years I have tried teaching under a considerable range of conditions; a variety of subjects in a one-room high school in northern Maine; English and Greek as a substitute in the high school from which I had recently graduated; English to immigrants in a New Haven night school; three summers of nature study to a group of youngsters outdoors in the Catskill Mountains; sub-freshman, at Kansas State College; laboratory instruction at Yale (this combined with some intensive and profitable tutoring); much later laboratory instruction in evening classes at George Washington University; finally graduate and undergraduate instruction at the University of Illinois, this last including two terms of ASTP geography. When, however, I undertook teaching general biology in our Division of General Studies I realized for the first time how hard teaching can be. It was some consolation to learn last spring that five different men had tried a comparable course at Columbia with reactions also comparable. For teaching such a course, I suggest as desirable characteristics, the wisdom usually attributed to Solomon, the patience which Job certainly needed, plus the god-given ability to sell the Brooklyn Bridge to the King of Siam.

Courses for General Education:— Finally, and briefly, what should be our objectives in elementary courses for students who, chiefly because they are not interested in anything in particular, are supposed to be seeking a general education? This subject is now much under discussion by those who already know the answers. Perhaps all the heat they are generating will shortly result in a little light. So far as any trend at all is discernible among these diverse councils, it seems to me to be in diametrically the wrong direction.

As I now see it, the importance of the subject matter, the importance of mastery of that subject matter, the actual memorizing, to use an unpopular word, increases as you go down (or if you like, up) the scale we are considering. It may make very little difference whether an advanced graduate student in entomology remembers or even learns the formula for D.D.T. He can easily look it up. Nor need every plant pathologist know the number of biological races of the cereal parasites—there will be more tomorrow. Nor can it be of great importance to pre-meds and pre-forestry students to remember the details of the anatomy of the salamander or the beech seedling. Reference books will be at hand. But unless the general student *really learns* that the process of photosynthesis is the present source of all our food and all our oxygen he has, in all probability, missed just that much, for he is unlikely to learn it later.

Perhaps we will not be able to go all the way back to AGASSIZ' methods but we might start in that direction. Suppose it did take BEAL three days to learn that a perch has a bilateral symmetry. He surely never forgot it and no doubt looked for bilateral symmetry in other animals. By contrast I have recently had students fresh from a year in high school chemistry, supplemented by a year of some other science, who had no idea that

$$6CO_2 + 6H_2O \rightarrow C_6H_{12}O_6$$

is *not* an equation. Some of them did know that $3 + 4 = 6$ is not an equation.

With the rapid expansion of knowledge in all our fields, it is obvious that the selection of material for beginning courses must be increasingly dras-

tic. Perhaps something might be learned at this point from our colleagues who have been teaching ASTP language courses. They have come to agree with St. Paul "I had rather speak five words with my understanding, that I might be able to teach others also, than ten thousand words in a tongue." (1 Corinthians 14:19.) Let the young soldier who might find himself in Germany learn, *really learn,* a few essential sentences such as:

"Ich bin hungrig," or

"Nudeln mit Rindfleisch freut mich ausserordentlich," or

"Dein Händchen ist wunderschön,"

and he will get by all right. The future-perfect subjunctive can wait until later. Our beginners may well be asked to *learn* something of the significance of the angiosperm seed. The alternation of generations in the *Rhodophyceae* can wait.

If it be objected that this type of instruction is difficult, I will remind you of the difficulty of the other method and particularly of the temptation it offers to merely amuse the student. Etkin, Kille, and Livingston[4] refer aptly to the danger that college teaching may "Fall to the level of the 'Oh my!' type of popularization of knowledge."

Freed from all restrictions of colleges, colleagues or conventions, and having in mind only the good of the students, I should certainly never offer a beginning student a general course in biology, or in botany, but rather a course in the green flowering plant, or in seeds, or perhaps in leaves.

From the above, it is clear that I feel that many of those who would improve our elementary courses for general education are trying to do far too much, more than we can do, more than our students can do or should be asked to try to do. I wish they too might search the scriptures: "First the blade; then the ear; after that the full corn in the ear" (Mark 4:28), or "Seek not things that are too hard for thee, and search not out things that are above thy strength." "Be not over busy in thy superfluous works, for more things are showed unto thee than men can understand. For the conceit of many hath led them astray" (Ecclesiasticus 3:21-23).

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[4] Etkin, W., F. R. Kille and L. G. Livingston, "A progress report of the Committee on basic concepts in Biology," *Graduate Record Examination Occasional Circular No. 2,* 1943.

— XVII —

THE PLACE OF PLANTS IN ELEMENTARY ECOLOGY*

Ecology is the central theme of this program. May I begin, then, by quoting from a botanist whom I often consult, one characterization of ecology.

"Ecology is not a science. It is a point of view. It is impossible to separate ecology from the other life sciences. It is impossible really to study any of the life sciences and neglect the ecological point of view."

Much—probably most—of my work during the past 35 years has been devoted to an attempt to utilize this point of view in plant pathology. The temptation to review some of this work is very great. In view, however, of the fact that we are here concerned chiefly with teaching, particularly at the secondary school level, it seems better to confine attention to more basic considerations.

What is more basic than that life continue? This is possible only if living matter is able to draw continually from the incomprehensibly vaster quantity of nonliving material about it enough of that material to replace itself. Everything else must be secondary. There can be no point in discussing the relation of living things to one another until their relation to the great inorganic sources of food and energy has been grasped. These reserves of raw material and of energy cannot be utilized directly by all organisms. Most of the O_2 in the universe is held in chemical combinations from which it is separated in significant quantities only by green plants. Not until it is thus separated is it available for respiration. So with food, not only the carbohydrates but most of the amino acids, even some fatty acids, and many of the vitamins must be synthesized in plants before they can be used by organisms which lack chlorophyll.

Moreover the sun is the ultimate source of virtually all energy which can be made available for living things. The generation now in school has heard much of atomic energy, not to mention atomic bombs. It might then be wise to point out in a beginning course in ecology that the sunlight equivalent of Uranium 235 in all known deposits is three minutes. The sunlight equivalent of all known coal is fifteen days. With exceptions

* Invitation paper before the National Association of Biology Teachers, Boston, Mass., December 28, 1946. — Published *in part* in *The American Biology Teacher* 9: 173-174 (1947).

which are surely negligible in most discussions of ecology, this energy of the sun is made available to living matter only through the agency of green plants. Green plants then and their relations already mentioned must constitute the basis of all teaching of ecology.

What has been said must sound to many of you like an exposition of the obvious. There seem to be some, however, to whom these things are so familiar as to be either forgotten or ignored. Last summer, in preparation for this session I read a number of texts on ecology, but in none of them did I find the sort of material we are now discussing. It was absent even from the opening chapters of elementary texts. Instead I found myself confronted with autecology and synecology, or with germules and propagules. No doubt dominance, stratification, quadrats, transects, and bisects all have their place. It is hard though, for one not of the elect to believe that their place is in the minds (or at least before the eyes) of secondary school students. Indeed it is hard to account for their presence there at all except for the tendency of the learned in all ages to pay tithe of mint and anise and cummin, and to omit the weightier matters of the law. (MATTHEW 23:23.)

By this time it must be evident that there seems to me but one place to begin the study of ecology. That place is the relation of green plants to their inorganic environment. That seems to me the best possible approach even if ones ultimate objective relates particularly to animals or even to special groups of animals including man. Ecology—by definition—is the study of organisms in relation to their environment. Green plants constitute one of the indispensable elements in the environment of all animals. A study of animal ecology must then begin with a study of green plants, not with studies of succession, or of climax formations, or of ecesis and aggregation, but with a study of the mass production of those basic things without which animals cannot live.

A logical next step would be to study those animals, like pollinating insects, which contribute directly to the welfare of the green plants on which we all depend.

Finally in regard to those animals which contribute only very indirectly or not at all to the welfare of green plants, one might well concern himself with how far they can get from green plants and still maintain themselves. That is, study some of those intricate "chains" by which the animal is connected with the ultimate source of its ability to live. A related problem is the question of how long some animals can remain away from the proximity of green plants.

Obviously most of this is more easily taught in the field than in the laboratory. It is more easily taught in the country than in the city. The preparations of animals (including man) for winter emphasize the changes which are necessary to maintain life when green, actively growing plants are no longer available. Plant products are stored as honey, as seed (including nuts), or as parts of stems buried in the ground. They are stored as hay, as roots and as silage. Some animals obtain plant food stored in twigs and dormant buds by browsing. Many birds, indeed many people, migrate to literally greener pastures. Hibernating animals store their reserve as fat and reduce consumption by sleep.

City dwellers have available the contents of tin cans, and the contents of huge cold storage warehouses. Recently frozen foods have become avail-

able to them. Every refrigerator car that rolls from a southern state to a northern market is moving food from a region where the leaves are still green to places where, out doors, only the conifers are green. I say "out doors" advisedly for the American term "green house" tells its own story. In its own way the extent of the green house industry emphasizes man's dependence on photosynthesis. I am assured on good authority that the cash value of the vegetables grown under glass during the normally dormant season in the state of Ohio exceeds that of the vegetables grown out of doors in the summer in the same state. Potatoes, of course, are not included.

Somewhere in even the most elementary discussion of ecology there must be a place to emphasize the fact that each organism has some effect on others, and that no organism can be added to or removed from a community without changing the life of others. It seems to me that in elementary courses this may well be deferred until toward the end. By this time it may have become evident, perhaps obvious, to the better students.

In reading various ecological works during the past summer I found repeated references to S. A. FORBES' classic paper "The Lake as a Microcosm."[1] All agreed that this was a milestone in the development of the ecological point of view. My first reaction was one of surprise that so late as 1887 it was a great advance in ecology to point out the interdependence of all living things within a limited space, and of their dependence on the nonliving elements in the environment.

I soon realized however that it was not until after 1887 that insect transmission of a plant disease was demonstrated. It was very much later than this that serious attention was first given to the study of the effect on plant diseases of so obvious a factor as soil temperature. I remembered too how often a plant pathologist has been content to end his study of a disease as soon as the causal organism has been identified and named. As a dweller in this glass house I am in no position to cast even a very small stone at the ecologists. On the contrary it seems to me to be the special function of the ecologist, now as in 1887, to point out to the beginning student and mature investigator alike that the living world is indeed one world.

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[1] FORBES, STEPHEN A. The Lake as a Microcosm. Two earlier editions of this paper have long been out of print. A third "with trivial emendations" is available in Illinois Natural History Survey Bulletin 15: 537-550, 1925.

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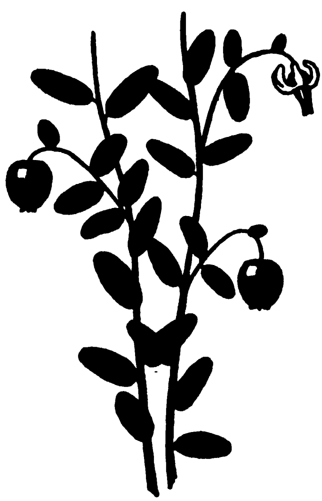

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FREEDOM
from
WANT

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WANT

A Survey of the Possibilities of Meeting the World's Food Needs

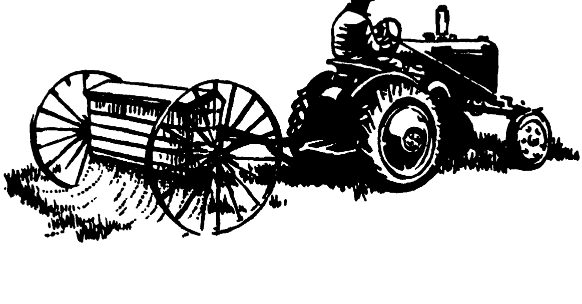
A SYMPOSIUM

edited by

E. E. DeTurk, Ph.D.

for the American Association for the Advancement of Science

With a foreword by Norris E. Dodd
Director-General, Food and Agriculture Organization of the United Nations



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FOREWORD

by

Norris E. Dodd
Director-General, Food and Agriculture Organization of the United Nations

When President Roosevelt included world-wide "freedom from want" as one of the great aims of the peace following the last war, he caught men's imaginations with a compelling slogan. More than half the world's people do not get enough to eat. When a man's stomach is never filled and a woman always sees her children hungry, the most inspiring promise is the promise of enough food.

President Roosevelt knew this when in 1943, in the midst of the war, he called the Hot Springs Conference on Food and Agriculture. This was to be the first step toward making freedom from want a world-wide reality. The conference itself was an inspiring thing, and those who attended it were full of hope and confidence in the future. They laid good foundations, too. On these foundations the first of the new United Nations agencies, the Food and Agriculture Organization, was built late in 1945. This is the organization through which the member nations—57 of them at this date—are endeavoring to work out ways and means by which the amount of food produced in the world can be brought nearer than it is now to what the human race needs, and the food can be made available to the people who need it.

FAO already has some worthwhile undertakings to its credit, but the battle has hardly begun. It will obviously be a long, hard, up-hill fight. The essence of it, as this book points out, is to increase the production of food faster than world population is increasing. For millions of human beings who are on the margin and never get enough to eat, this means a race with death. So far they have been the losers. These marginal millions die young, and during their short existence the fire of life burns too low in their bodies for health and full achievement.

Sir John Boyd Orr, the first Director-General of FAO, has said many times that unless we can speedily begin to gain in this race between population and food supply, we face widespread disaster. The present rapid growth in world population—there are now at least 150 million people more than there were ten years ago—and the steady loss of soil productivity through erosion and bad management means that the human race will go deeper and deeper into the red on food; and this growing deficit in turn means that there will be increasingly bitter rivalry between nations for the earth's basic resources. In an age of atomic and biological warfare, disaster must be the ultimate end. Whether or not you consider this view too gloomy, it is certain there can be no lasting peace in a hungry world. Hence food—the production of food, the distribution of food, and the use of food—is becoming the great preoccupation of our time.

We in FAO are keenly aware how difficult it is, first, to develop programs adequate to bring about increases in food production and improvements in distribution of the magnitude the world needs; second, to get nations to adopt the vigorous national and international policies necessary to carry out such programs. These difficulties are enormously increased if doubt is cast on the value of such efforts in the first place. There are plenty of people, sincere and able people, who have such doubts. They can "prove" that the world's productive resources are and will forever remain inadequate to provide good nourishment for more than a comparatively small part of the human race. They can "prove" that human procreation will continue at such a rate that the great masses of humanity must always be uncomfortably near the edge of starvation. People who read and accept these statements, especially when they are made by authorities, naturally wonder whether it is worthwhile to make such efforts as those to which the Food and Agriculture Organization is dedicated.

And if this view really represents the truth, it is not worthwhile. Mankind in that case is caught in the grip of a blind fate which is pushing him on to destruction, and he can do nothing effective to prevent it. The struggle for food will become more relentless; nations will always be snapping at each other like coyotes snarling in the dark of the moon over a sheep's carcass.

But this view does not necessarily represent the truth. There is ample reason to believe that man can be much more master of his fate than these extreme pessimists think. I would agree that it looks very much as though the road along which the human race is driving right now leads to a dead end—and I mean dead. But I would not agree that this is the only road mankind can follow.

First, by vigorously applying our present knowledge, we can make the earth and its waters produce far more for our sustenance and comfort than they do today or ever have in the past.

Second, history indicates that the growth of scientific knowledge, making possible still greater production, is not likely to stop with this current fiscal or calendar year.

Third, men and women are not vinegar flies, however useful the latter may be for population studies. The development of great modern industrialized nations tends to show that human beings, endowed with the ability to understand and to weigh values, can modify the dictates of that simple biological law which compels procreation, insectlike, up to the uttermost limits of the available food supply. After all, human understanding is itself a biological phenomenon, and if it is given a chance to develop it is bound to make a difference in the way people act. It cannot develop very far, however, when men and women are driven down to the most primitive level of living by poverty and hunger.

Because this little book says such things as these and much more, I think it is important. It says these things clearly and dispassionately, with scientific detachment. The authors, scientists and economists, ask the question: Can mankind achieve freedom from want? They have tried to discover the truth, and they come out with an affirmative answer.

I would like to emphasize that as far as present scientific evidence goes, the answer in this little book is at least as valid as the negative answers of the pessimists—and that is a deliberate understatement. This positive answer has one great advantage. "From such an interpretation," as Professor BLACK says in his paper, "one is able to arise with vision of hope for mankind." Hope is necessary if you expect to get anything accomplished.

Civilization, it seems to me, is in somewhat the position of a man in an automobile of ancient vintage which is stalled on a railroad track with an express train whistling just around the bend. He can stick to the old car, persist in trying to get it started, and end in the morgue. Or he can jump and go on about his business. He will lose a second-hand automobile, but he needs a new one anyway. If he expects to go on living for happier days, however, he had better make up his mind and act quickly.

The message this book conveys is that civilization can get to safety; it need not be smashed in a desperate struggle of too many people to live on too limited resources.

SALTER shows in a boldly reasoned argument that by using our present cultivated lands better and bringing certain new lands into use, the nations could double their food production by 1960—in other words, produce enough food for all people to reach the goals of better nutrition suggested by FAO in its World Food Survey.

BLACK shows that as undeveloped communities and nations modernize their economies, the rate of population growth can be brought within more reasonable limits.

TOLLEY points up the fact that we can achieve such results as these if, and only if, we pool the knowledge and effort of both the physical and the social scientists in a concerted world-wide effort.

QUISENBERRY tells some of the things that have been accomplished in increasing crop production in the more advanced countries by way of indicating future possibilities elsewhere. "These figures are almost unbelievable," he says, in commenting on the great increases in the yield of rice per acre in the rice-growing areas of the United States, "yet they show what applied research might accomplish with this crop on a world basis." He points out that an increase of only 10 percent in yields in China and India, which is an entirely reasonable expectation, would supply food for some 58 million people at present average consumption rates.

MORRISON writes about the relative efficiency of various farm animals in supplying food for human beings and discusses some of the possibilities of increasing production.

McCALL emphasizes the urgent need for better, quicker ways of getting scientific knowledge about improved land management into widespread use on farms.

Of course these writers say much more, though their book is brief. But it is clarifying. And it is especially significant because it grew out of discussions at a recent annual meeting of the American Association for the Advancement of Science. I hope to see scientists give more and more con-

sideration, in their meetings and elsewhere, to the great world problems of producing and distributing food and other products of farms, forests, and fisheries.

Through FAO, and some of the other economic organizations of the United Nations, governments have committed themselves for the first time in history to a concerted, world-wide effort to meet the needs of mankind for the fundamental necessities of life. It is a gigantic undertaking, with vast implications for new developments in industry, agriculture, forestry, and fisheries. I know of no international effort more pregnant with hope for the future.

This movement grew directly out of modern science. It is essentially an attempt to spread to all man the usages and the benefits of science applied in many fields. The help of scientists will be needed at every step in carrying it out.

WASHINGTON, D.C.
August 1948

FOOD FOR FREEDOM

EDITOR'S PREFACE

There was a time when agriculture consisted of the nomad seeking pasture for his herds and flocks. From this humble beginning the art of farming developed through the centuries. Perhaps the peak of this stage of development is epitomized by the statement, not uncommon a century ago, that a man who had failed in all other pursuits could farm. The development of a modern agriculture which could persist in the present technological age has been dependent upon the gradual and at times difficult process of placing under it a solid foundation of Science.

Perhaps no other industry today requires the contributions of so many of the Sciences—physical, chemical, biological, economic, and social. Specialists in all of these divergent fields may be found today on the research and teaching staffs of our agricultural colleges and experiment stations.

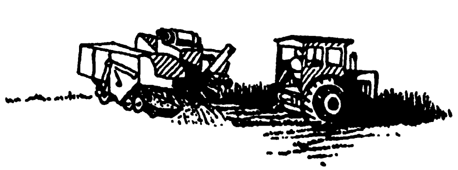
One of the objectives of the officers of the Agriculture Section of the American Association for the Advancement of Science is to bring together scientists from divergent fields of research in order to pool the results of their work for the benefit of Agriculture and of the public. To this end the symposium represented by this volume was arranged during the December 1946 Meetings of the Association at Boston, on the theme of "Freedom from Want," with special reference to food.

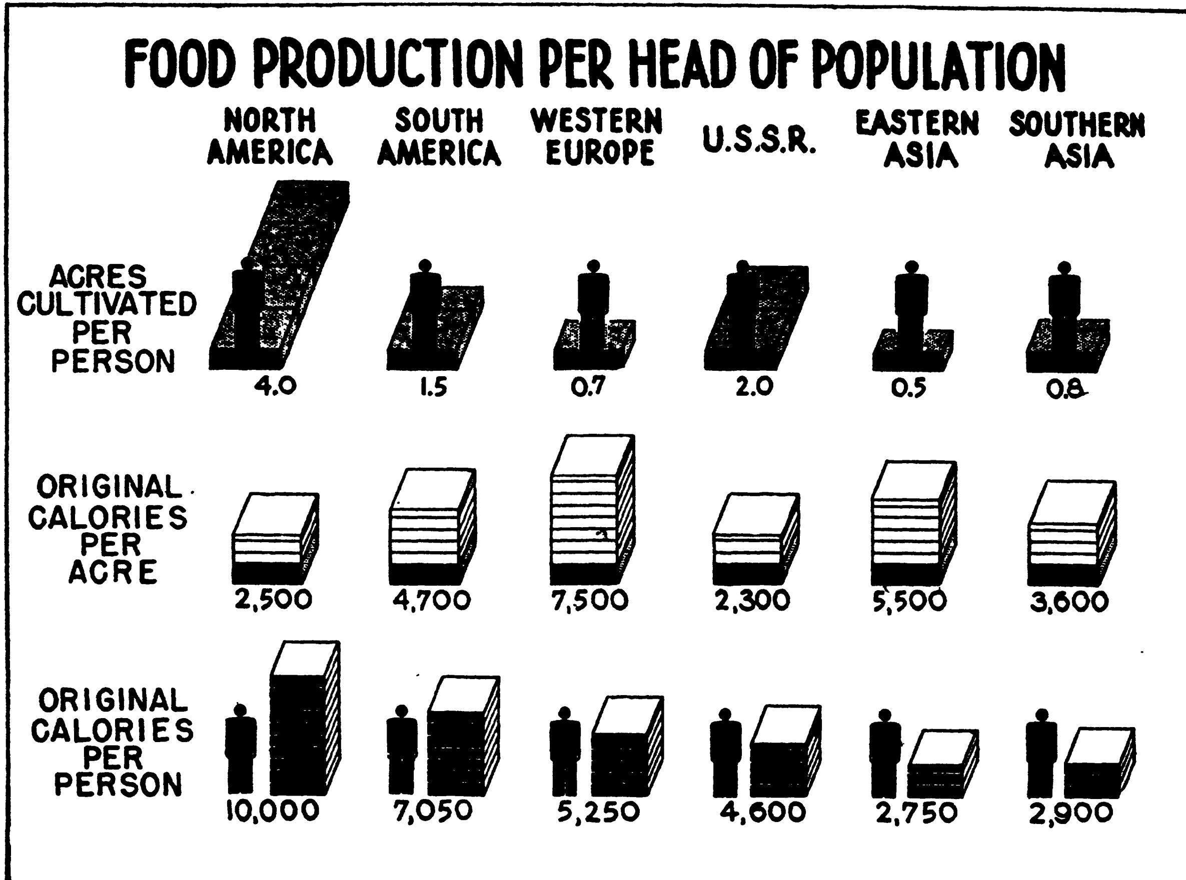
Authorities in different areas of research were invited to participate by presenting authentic information on the problems involved in feeding the world's people. How many people are there to be fed? How nearly adequate are their present diets? Can the world's soils grow enough food? Where are these soils? How completely are they now developed? Are the known fertilizer reserves adequate for full development of soil productivity, and so on.

These and other questions must be answered including the possibilities in the development and maintenance of production of food crops and livestock products as well as the economic and social problems of both production and distribution.

It is too ambitious to hope that the present work will solve these problems but it is the hope of the authors that it will aid in clarifying the many problems and in suggesting avenues of approach to their eventual solution.

E. E. DeTurk




FOOD PRODUCTION PER HEAD OF POPULATION
NORTH AMERICA
SOUTH AMERICA
WESTERN EUROPE
U.S.S.R.
EASTERN ASIA
SOUTHERN ASIA
ACRES CULTIVATED PER PERSON
4.0
1.5
0.7
2.0
0.5
0.8
ORIGINAL CALORIES PER ACRE
2,500
4,700
7,500
2,300
5,500
3,600
ORIGINAL CALORIES PER PERSON
10,000
7,050
5,250
4,600
2,750
2,900

POPULATION AND FOOD SUPPLY

by H. R. Tolley

Director, Economics and Statistics, Food and Agriculture Organization of the United Nations

Modern science has given us new tools with which to meet the age-old problem of balancing population and food supply. In those parts of the world where science has been most extensively applied to agricultural technology there have been significant increases in production without corresponding increases in the land under cultivation. A new variable has been introduced into the relations of rates of increase of population and of food supply. Enough food of the right kinds for all the people is no longer only an Utopian's dream; science has given us a basis for confidence that the achievement of this goal is within our reach. The people everywhere, alert to the new opportunities presented by rapid technological advances in agricultural and related sciences, want bold action to get this job done. Governments have shown a readiness to coöperate with each other in the application of the new knowledge to problems of food and nutrition. The stage is set for greater efficiencies in producing and distributing food from the approximately 4 billion acres of land on which the world's agriculture rests.

The problems of increasing food supplies more rapidly than population and thus improving the nutrition of people is a complex one, which requires the best efforts of science, biological, physical and social. And much more scientific development is needed. Nutritionists need to expand their research on mankind's dietary needs, the ways in which they can be met and the consequences of failure to meet them. The agricultural sciences need constantly to push back the frontiers of knowledge of the factors basic to production of food. Economists and other social scientists need to find ways of improving the means by which the benefits of increased production can be made available to producers and consumers all over the world. Policy makers and practical men of affairs are needed to develop the policies and the organizations necessary if the potentialities which science offers us are to be realized.

With the best efforts of all concerned, of course, an integrated world food policy will not be attained overnight. But the more quickly bold attempts are made to achieve it, the sooner the world will be rid of the distress of its millions of poorly fed people, its bankrupting food surpluses, and the constant threat of economic and military competition for available land.

My purpose in this brief paper is to present the problem in very broad outline, emphasizing the possibilities for immediate action, as well as longer-term issues.

There are two points which, at the risk of seeming repetitive, I shall stress now, and also later on. Point number one: the heart of the problem lies in increasing production faster than population growth.[1] Only by providing each person with the scientific and technological tools which

enable him to become a new source of wealth to the world can food supply ever be brought into balance with population.

' Point number two: only through a broad, coordinated attack involving every phase of the scientific, industrial, financial and agricultural economy can this be accomplished. '

World food consumption:— A logical starting-point for discussing the population-food supply relationship is afforded by our present knowledge regarding the world food situation. It is an old story that millions of people in the world do not get as much food as they want and need for minimum health. But only during the last quarter of a century has the science of nutrition made it possible to measure this shortage with some precision.

Nutritional deficiencies do not always manifest themselves in the better-known deficiency diseases, such as pellagra or rickets, although their incidence is high in some countries with relatively low levels of nutrition. Vitamin deficiencies are possibly related to the high rate of ophthalmic diseases in some areas. Nutritional deficiencies are normally associated with deficiencies in housing and in health services. The combination is reflected everywhere in lowered vitality, high rates of morbidity and a high rate of wastage of human life. Experience has shown abundantly that high rates of infant mortality and short expectation of life are closely associated with inadequate nutrition.

One measure of nutritional levels is the extent to which the people of a country are dependent on certain foods.

We know, for example, that when a nation depends on potatoes and cereals for 80-90 percent of its calories, many of its people can be assumed to suffer from mineral and vitamin deficiencies. If, on the other hand, it derives 35 to 50 percent of its calories from high-protein animal products, there is far less likelihood of these deficiencies. Using this knowledge, it is possible to bring the prewar diets of individual nations into fairly clear focus.

Only a handful of nations, with a small part of the world population, enjoyed comparatively good diets before the war. They included the United States, Canada, Australia, New Zealand, the United Kingdom and such northern European countries as Holland, Sweden and Denmark. The United States relied on cereals and potatoes for only 30-40 percent of its calories, while 35-40 percent were obtained from animal products (including meat, fish, eggs, milk, butter and lard). In the United Kingdom, the cereals and potatoes also furnished 30-40 percent of total calories, while animal products provided 40-45 percent. The same dietary pattern is evident in the other nations with relatively favorable food intake.

This favorable balance of calories is in sharp contrast to the situation in many other countries. Italy, for example, got 60-70 percent of its calories from cereals and potatoes, and only 10-15 percent from animal products. In Egypt and the Middle East, the population relied on cereals and potatoes for as much as 70 to 80 percent of its calories, obtaining only 5 to 10 percent from animal products.

In the Far East, many regions of which have skirted the edge of famine for centuries, the situation could properly be described as desperate. China's people derived up to 90 percent of their calories from cereals and starchy roots and only 1 to 5 percent from animal products. Diets were little better

in Japan. Although nutritional data from these nations are scanty, there are evidences of serious deficiencies in both.

The populations of most of Africa and Latin America were little better off from a dietary standpoint. Tunis, Algeria and French Morocco all obtained 70-80 percent of their calories from cereals and potatoes. Chile depended on these foods for 60-70 percent of its calories, getting only 15-20 percent from animal products.

Statistics indicate that even in the best-fed nations, wide geographic and economic variations in diet existed.

Looking at the picture in broader terms, we see that prewar diets in areas containing over half of the total population had food supplies insufficient to provide at the retail level more than 2250 calories per person each day. Countries with less than a third of the population received more than 2750 calories per person daily. The rest of the world, with one-sixth of the population, had diets between these two extremes.

The significance of these figures can best be shown by quoting a sentence from a report of nutrition experts who concluded that, "a per caput calorie intake of 2550-2650 should be taken as the minimum level to which intake should be raised in the low-calorie countries. . . ."

A world food appraisal for the 1946-47 consumption year indicates that these contrasts in diet still persist. While the United States and certain other exporting countries are eating better than before the war, large parts of Europe and Asia are getting only 60 to 70 percent as many calories per person as prewar. Large groups of people in some of the nations have less than 1000 calories per person daily. The best-fed peoples are consuming more meat and milk, while those in the deficit nations are obtaining a smaller proportion of their calories from fats and animal products. Nutrition and health in most of these nations are as bad as or worse than prewar.

Although the current food crisis is due in part to transportation difficulties, price rises and materials shortages, the chief long-term cause of inadequate diet and malnutrition is poverty. The countries in which the prewar calorie supply per caput was less than 2250 a day were those with an average per caput income of less than $100 a year. Most nations with calorie supplies of over 2900 a person daily had per caput incomes of more than $200 a year. Poor people cannot buy enough of the right kinds of food and poor nations cannot finance the necessary expansion in production to lower the cost of food.

World food needs in 1960:— Obviously, consumption of protective foods such as milk, meat, vegetables, eggs and fish would have to be materially increased to meet the minimum dietary needs of the prewar population. But population has increased by about 7 percent since 1935-39, and all available data indicate a continued increase in the immediate future even after the current postwar boom in birth has ended. The main job, as I see it, will be to supply more food to meet this quantitative increase, and to feed all the people better at the same time.

A recent publication of FAO provides a rough measure of the size of this job. In preparing it a group of nutrition experts set up nutrition "targets," *i.e.*, conservative estimates of the nutritive needs to maintain minimum health standards. In formulating the targets, attention was given to the pattern of national diets and the levels of nutrition prevailing in the countries before the war. Since the attainment of the goals of better

nutrition was recognized as a long-range process, the year 1960 was selected for the comparisons.

Even if no gain in nutritive levels between the prewar years and 1960 is assumed, it is necessary to provide for an increase in food production of approximately 25 percent, for the world's population will probably have increased by at least that much by that time. In the most poorly nourished areas—India, Southeast Asia, parts of Southern Europe and Central America—population is growing more rapidly than in most other parts of the world and it may well be that an increase of the order of 35 percent in total production would be necessary to maintain their food supplies at the same level per person as before the war. For these areas, the total increase in the production of original calories needed to provide for the increased population and for improvement in nutritional levels to the "targets" amounts to 90 percent. In other words, for these areas it would be necessary nearly to double the production of original calories above prewar levels.

For the world as a whole the improvement of nutrition to the levels assumed to be necessary requires large increases for many commodities. For fruits and vegetables an increase of 163 percent is needed, for milk the increase is 100 percent. For pulses it amounts to 80 percent and it is 46 percent for meat, 34 percent for fats, 27 percent for roots and tubers, 21 percent for cereals and 12 percent for sugar. Though these seem to be very large increases, I can assure you that our computations of needed increases have more often been criticized as being too low rather than as being too high.

World food production:— This brings us to the production side of the food-supply picture. Today, over 90 percent of the world's food is consumed in the countries where it is produced. Yet, less than one-fourth of all the farmers have begun to apply the techniques of modern science. If the gap between food supplies and requirements is to be eventually closed, technology must constantly improve and be made available to all producers, whether they are poor or well-to-do.

I for one have no doubt concerning the ability of science to measure up to the job. Agricultural science is relatively young. Yet, it has already effected what is properly termed a "revolution" in farming in all those regions which have felt its influence. For example, in England, the aid of science has caused a steady increase in wheat yields during the last century. (In some countries, a rise of 50 percent during a decade has not been uncommon.) The yields of all root crops have moved upwards despite a reduction of the arable area. The weight of carcasses and the yield of milk have also risen. The story is much the same in many other countries. Others this morning will no doubt tell you that we are just on the threshold of further significant technological advances.

One need only consider what happened in the application of scientific farming during the war. Many factors combined to enable farmers to produce more food in countries which were not the scene of armed conflict. Here in the United States the production record is one that indicates the potentialities. Total agricultural production has been about a third greater than before the war, with about 1/7 less manpower being used. But fertilizer use was nearly doubled, more farmers used insecticides and machinery, and the use of some of the larger machines was spread over larger

areas and longer periods of time than before. The use of hybrid seed corn became virtually universal in the commercial corn-growing areas, and improvement in varieties of crops and of livestock went on rapidly. There were also some improvements in the methods of storage, and reimprovements in the efficiency of transportation continued to be made. A part of the wartime record of production in this country was due to the fact that we entered the war with large stocks of wheat and feed grains. And the weather has been unusually favorable. Nevertheless, the United States is not likely to be satisfied again with levels of agricultural production like those before the war.

The record in the United States indicates what can happen in an area where production levels and productivity are already high. When the impediments to production which result from the war and postwar dislocations are finally removed, there should be rapid increases in agricultural production above prewar levels. It is not difficult to visualize the increases that would be possible in some areas of low productivity if even moderate improvements in agricultural technology were to be generally adopted.

Such a production increase, however, will be realized only if: (*1*) the disadvantaged nations are enabled to obtain the technological and scientific tools they need; and (*2*) consumers everywhere can buy enough of the right kinds of food at a price which will enable producers to live.

The underdeveloped countries constitute about ⅔ of the 60 separate nations. They are inhabited by 1500 million out of the world's 2100 million people. They need capital to finance their own agricultural expansion. Although a comprehensive inventory of the soil resources of these nations remains to be made, it is known that they contain millions of unused acres which might, through proper scientific management, be made productive.

A program of development for these regions might well start off with the badly needed soil inventory. Simultaneously, the present and future world demand for fertilizers should be determined, and fertilizer-producing nations encouraged to expand their output.

Better tools should be provided for the more adequate processing, handling and storage of agricultural products locally consumed, such as the milling of rice, the more sanitary handling of dairy products and the preservation of foodstuffs and fodders. War-devastated nations should be enabled to restore their own machinery factories.

Improved varieties of seeds, suited to the climatic conditions of each area, should be introduced in the disadvantaged nations. Production methods generally can and should be greatly improved, including better livestock-breeding methods, adoption of soil erosion practices, better roads, and the development of reserve stocks of vaccines to combat livestock diseases.

The international implications of such a program are fairly obvious. Funds must be made available to nations requiring credit to finance these improvements — perhaps through the creation of coöperative farm credit associations. If a particular nation needs food imports, exporting countries must expand their output to meet this demand. Simultaneously, the importing nation must boost its production of other goods to trade for the food it imports. Price changes which might discourage farmers from adopting improved methods must be avoided.

But the problem is not only an agricultural one, susceptible to solutions within the framework of agriculture. Land resources everywhere are limited. Rural populations tend to increase more rapidly than urban populations. In many of the countries of the world with low nutritional levels there is acute pressure of population on the land, with prospects that unless some alternative opportunities are found, this pressure will be increased. When population presses too heavily on the resources of the land, rural underemployment and inefficiency are inevitable. Human abilities stagnate during a good part of the year, and the rewards for human labor fall far short of what they might be. The output of food per man is ten times greater in the advanced than in the poorer countries. The conclusion is inescapable that the food of the world can be produced in much greater abundance by fewer hands. In fact, unless some way is found of reducing the number of hands trying to gain a subsistence from a relatively underdeveloped agriculture, the way to achieving the production and nutrition levels which are potentially possible may be barred.

The way out of this situation is to open up resources other than those of farming for the bulk of the population. The opportunities for the use of human skill through the application of modern science and technology are enormous. By developing these opportunities, the way will at the same time be opened for those remaining on the land to increase their efficiency manyfold.

This calls for rapid large-scale development of industry and trade, and of educational and other services. For that purpose large investment both of capital and of technical skill will be needed. The only alternative to this investment for the western world is to restrict its own high production.

This is no small problem calling for halfway or partial measures. Rather it is a problem which calls for full-scale measures on a broad front. A little amelioration here, a half-hearted attempt at improvement there will serve in the future, as they have in the past, only to increase the numbers of the poverty-stricken and ignorant.

Population growth:— For some countries with high rates of population growth and limited resources, migration of part of the population may offer a temporary way out, especially if it is coupled with alterations in the patterns of population growth. This happened in Europe, where emigration to the North American continent provided a major outlet for population during the transition between declines in the death rate and declines in birth rates. Nonetheless Europe quadrupled its population in the 3 centuries between 1650 and 1940. But for the world as a whole, migration functions chiefly as a means of adjusting population to resources in local areas. It does not necessarily affect the relations of people to resources as a whole, and it may, under some circumstances, serve primarily to spread the misery of already crowded areas into areas where the balance of population and resources would permit the development of high levels of production.

The world's population, which stood at about 500 millions in 1650, had quadrupled by 1940. Rates of growth however were very uneven in different parts of the world. At present, potential rates of growth, assuming a continuation of the long-time trends which were in evidence before the war, differ widely, depending on the degree of adaptation to the conditions

brought about by rapid industrialization and the accompanying changes in attitudes toward mortality and fertility.

The demographic transition from high mortality and high fertility to low mortality and low fertility is well advanced in the nations of Western civilization. Within a generation, relative stability or actual decline may characterize the populations of the economically advanced industrial and urbanized nations, including northern and western Europe, the United States, Canada, Australia, New Zealand and the white population of the Union of South Africa. Taken together, these nations include less than one-fifth of the world's population. Areas in which the transition from high to low fertility and mortality is well advanced, but which have a potentiality for substantial growth for at least a generation or two, include the Soviet Union, Japan, eastern and southern Europe and parts of Latin America. These areas contain another fifth of the world's population. Thus the areas in which industrialization has moved forward farthest, and in which population growth may have ceased by the end of this century, include less than 40 percent of the world's population.

In contrast stand the great agrarian regions of Asia and Africa, with three-fifths of the world's population. Western penetration during the last century has meant relative political stability, the extension of agriculture and minimum standards of epidemic control and famine relief. The demographic effect has been to control mortality, but in few cases have the standards and values which might limit fertility been as widely or as rapidly diffused. The population of Asia outside the Soviet Union increased by about 30 percent between 1900 and 1940.

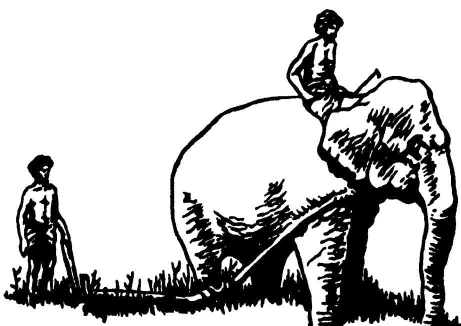
There appears to be no possibility for minimum living within agriculture for the increased population that would result in this area from the introduction of the agricultural improvements necessary to adequate nutrition and other elements of living. If industrialization occurs and if population growth follows the patterns of Europe, America, Oceania and Japan, fertility will fall as urbanization and its correlated habits of living and thinking are accepted by increasing proportions of the total population. But unless the process is more rapid than it was in these other areas, truly prodigious increases in agricultural and industrial production would be required to provide for the population increases that would occur while fertility is being adjusted to lowered mortality. Conversely, unless increases in production do occur, the decline in mortality rates most likely will be halted, and increases are almost certain to occur. The whole cycle of industrialization and decline of fertility in Japan followed the western pattern, but at a more rapid rate. But even the rate of Japan is not rapid enough by itself; the real demographic need of Asia is to devise ways by which the diffusion of the small family pattern among the peasants may be quickened.

In estimating the future population of Asia or Africa, it is not possible to take population growth as the independent variable. Rather the rate of increase will be largely determined by the food and other resources, and by the knowledge and the medical facilities available for the support and preservation of the population. If agriculture is improved and control of famines and of epidemics and other health hazards is achieved, population might continue to grow for some time. But eventually declines in fertility or increases in mortality would be imposed through the interaction of other

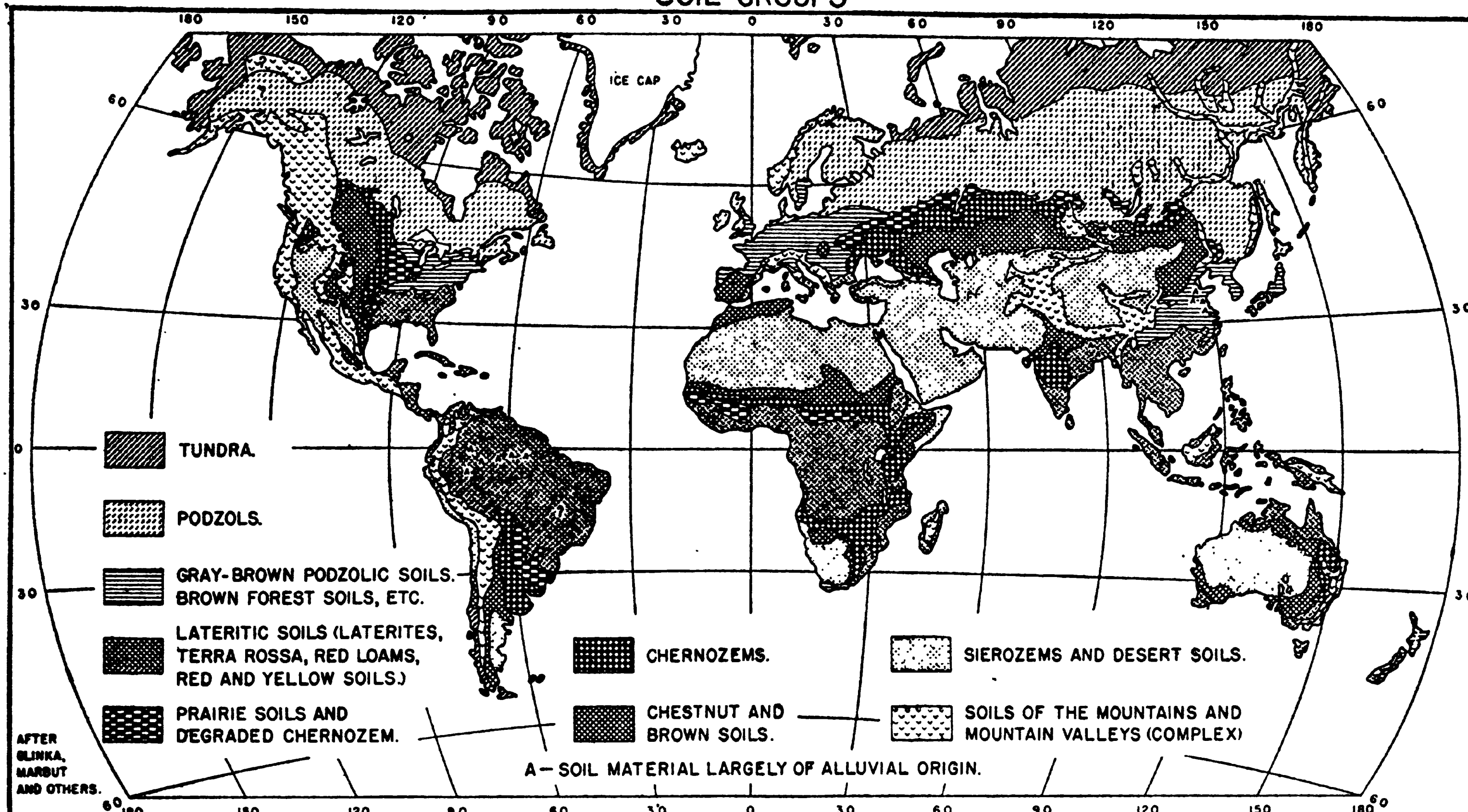
factors. In Western countries rates of population growth have declined as standards of living have gone up. A social environment was created in which parents consider it of paramount importance that their children should be well educated, vigorous and healthy and should have good prospects in life, and emphasis is placed on the development of facilities and ways of living that bring these objectives within reach.

No one line of development will be sufficient in meeting the problems of the areas that have not experienced this change in values. The need is for coördinated development and change in a society and its structure in all the aspects which bear on the attitude toward life and reproduction. These include trade, education, industry, health, social welfare as well as agriculture. We need more knowledge of the steps that might be taken as well as information on the mechanism of fertility control, especially in the familistic societies of the rural Orient, on the social psychological background for control in fertility, and on ways in which a 3 or 4 fold increase in already heavily populated areas might be avoided. The agricultural scientists and the demographers, economists and sociologists, as well as psychologists and many more have contributions to make here.

But action cannot wait until all the problems on which more knowledge is required have been solved. The job of providing better nutrition for the population of the world is a part of the job of breaking through the dilemma that seems to be inherent in the relations of population growth and food supply in large parts of the world today. It is not an easy job, and it is one that will require the best that civilization has to bring to it. Nothing short of a combined attack on all fronts will suffice to accomplish it. Through a new spirit of international coöperation the means are being developed for a pooling of all the skills in research, education, production, marketing, finance and related fields, and thus new tools for this age-old problem are placed in our hands.



SCHEMATIC MAP OF IMPORTANT
SOIL GROUPS



WORLD SOIL AND FERTILIZER RESOURCES IN RELATION TO FOOD NEEDS

by ROBERT M. SALTER

Agricultural Research Administration, U. S. Department of Agriculture

HUNGER and starvation have stalked the footsteps of man since the dawn of history. Only with the bloom of modern science in the last half-century has there come, for the first time, hope that these gaunt spectres could at last be banished if man but willed it so. Through the centuries those in positions of leadership have been prone to accept the misery of hunger as an unavoidable fact of life, the result of population pressure on a limited food supply.

In the midst of the most destructive war in history, the leaders of the Allied Nations determined that one of the goals of victory should be *freedom from want*. To some, science appears to offer a basis of fact for this ideal. Here, in our own country, farmers applied the results of agricultural research so efficiently during the war years that production of food crops — despite many difficulties — reached and held an unprecedented peak. And it seems reasonable that with the peace, benefits of agricultural research might be still further extended, and that all the people of the world might possibly be better fed.

The high ideals that came with the fervor of the war are now being reconsidered and evaluated coolly in the light of peace-time realities. The peoples of the warring nations are determined to be practical. With skeptical minds they are asking: Is it, after all, reasonable to hope that the world can produce enough to feed all its human inhabitants? Can the world's soil grow all the crops that would be needed? Are fertilizer sources great enough? Do we have the technology and management ability to produce the crops and maintain the soils?

These questions do not encompass the whole problem by any means, but these are the questions I am asked here today. I am glad to try to answer them, for I am convinced that we do have the soils we need, we do have the fertilizer resources, we have available the management ability, and we could produce enough food for all.

But how much is enough? We have an answer suitable for our purpose from the recent "World Food Survey" made by the Food and Agriculture Organization of the United Nations. By 1960, if everyone is to have an adequate diet, the world will need, according to the estimates of this survey, the following increases beyond prewar production: Cereals, 21 percent; roots and tubers, 27 percent; sugar, 12 percent; fats and oils, 34 percent; pulses and nuts, 80 percent; fruits and vegetables, 163 percent; meat, 46 percent; and milk, 100 percent.

The courses to pursue:— Considering only the natural physical resources needed to obtain these increases in food production, and with

acute awareness of many other problems involved, there are two obvious courses we can follow in seeking higher food production the world over. First, and perhaps as the easier course, we could obtain much of the increase through more intensive and more efficient use of the land now farmed. Second, with our knowledge of world soil types, we could expand production in the areas having undeveloped soil resources. I shall discuss these two courses separately and in the order named.

TABLE I: *Estimated attainable increase in acre yield due to improved practices:* —

Crop	Yield per acre 1935-39 (bu.)	Yield per acre 1960 (bu.)
U.S.S.R.		
Wheat	10.0	12.0
Rye	12.7	13.5
Corn	16.3	20.0
Oats	22.2	28.0
Barley	14.9	18.0
Sugar beets	6.1 T	8.0 T
Potatoes	121.5	180.0
India		
Wheat	10.7	20.0
Rice	26.2	40.0
Corn	12.9	20.0
Barley	16.5	20.0
Peanuts	400.0 lb.	600.0 lb.
China		
Wheat	14.9	18.0
Rice	52.5	70.0
Corn	24.2	35.0
Barley	21.8	24.0
Peanuts	769.0 lb.	1,000.0 lb.
Soybeans	16.8	20.0
Dry beans	730.0 lb.	1,000.0 lb.
Potatoes	100.0	150.0
United States		
Corn	28.1	36.7
Oats	31.7	38.5
Hay	1.4 T	1.8 T
Potatoes	124.0	152.0
Soybeans	18.5	21.9
Peanuts	765.0 lb.	916.0 lb.
Wheat	12.4	14.6
Rice	47.5	53.6

Intensify production on present acreage:— Some of the possibilities of intensive and efficient production are readily evident in our own war experiences. Production of food crops was maintained at about 35 percent above the period 1935-39. Admittedly, weather was more favorable in the war years than during the prewar years, but even so with no more favorable

TABLE II: *Prewar food production and increases attainable from more intensive use of present crop land*[1]: —

	Cereals	Roots and tubers[2]	Sugar	Fats and oils	Pulses and nuts	Fruits and vegetables[3]	Meat	Milk
	(millions of metric tons)							
Prewar production	300.4	153.2	30.0	15.2	36.2	156.3	65.6	150.2
Increase attainable from present crop land	20%	50%	15%	20%	20%	35%	20%	20%
Attainable production from present crop land	360.0	230.0	34.5	18.0	43.4	211.0	78.7	180.2
World food needs in 1960	363.5	194.5	33.6	20.4	65.2	411.0	95.8	300.0

1 For 70 countries including 90 percent of world population. World consumption of each class of food as given in "World Food Survey" of FAO is assumed to equal world production.
2 Include bananas.
3 Include eggs and fish.

TABLE III: *Potential food production from more intensive use of existing crop land plus development of additional land not now cultivated*: —

	Cereals	Roots and tubers	Sugar	Fats and oils	Pulses and nuts	Fruits and vegetables	Meat	Milk
	(millions of metric tons)							
Attainable production from present crop land	360.0	230.0	34.5	18.0	43.4	211.0	78.7	180.2
Attainable production from present crop land plus one billion new acres tropical soils[1]	717.5	469.5	177.5	69.5	55.4	470.0	89.4	188.8
Attainable production from present crop land plus 300 million new acres land outside tropics[2]	395.5	296.0	35.1	19.4	44.2	211.0	86.1	314.6
Attainable production from all above sources	753.0	535.5	178.1	70.9	56.2	470.0	96.8	323.2
World food needs in 1960	363.5	194.5	33.6	20.4	65.2	411.0	95.8	300.0

1 Obtained by applying the approximate average production per crop acre in the Philippine Islands to 1,000,000 acres.
2 Obtained by applying the approximate average production per crop acre in Finland to 300,000,000 acres of Northern Hemisphere soils. Fats and oils and fruits and vegetables are underestimated because Finnish production figures on farm-made butter, meat, fruits, and vegetables were unavailable.

weather the production would have been 20 percent greater, despite the fact that the labor force was actually 6 percent smaller.

Our experiment stations and our more successful farmers provide many illustrations of the opportunity for increasing food crop yields and animal production if these improved agricultural techniques were more widely applied. Thus, the limiting factors to increased production seem to be lack of education and lack of capital rather than any limits of physical production capacity. For example, recent experiments on corn culture in the Southeast under ordinary farm conditions show that corn yields for that area can be more than doubled by a combination of improved practices. In addition to regular fertilization practices, these include heavy nitrogen treatment combined with the growing of adapted hybrids, closer spacing to take advantage of heavier fertilization, and early and shallow cultivation for weed control. By 1960, perhaps, 50-bushel-per-acre corn production in the Southeast will be the rule rather than the exception as today.

This, and many other examples, would indicate also that the estimates of production possibilities after the war under prosperity conditions, made coöperatively by the United States Department of Agriculture and the Land Grant Colleges, are really conservative, though they may appear somewhat optimistic. This study estimated that there can be readily attained by 1950 appreciable increases in production per acre of most of our principal crops, over that obtained in the 1935-39 period. These estimated increases were as follows: Corn, 31 percent; hay, 28; wheat, 18; rice, 13; peanuts, 20; sugar beets, 17; potatoes, 22; and sweet potatoes, 31 percent.

But the United States alone cannot feed the hungry of the world. We must look to other countries also to intensify food production, preferably in the food-deficit areas. Europe, with the exception of Poland, Russia, and the Balkans, had reached a high degree of intensified farming in relation to its soil resources prior to the war. Significant increases in the more highly developed European countries are thus doubtful. If, however, we take into account yield increases readily attainable in different soil regions of the United States, we can safely predict yield increases in other countries that possess the same Great Soil Groups. In Table I we have such predictions for China, India, and the Soviet Union, the world's three most populous countries. These predictions take into account the present reported average yields in these countries and also the general intensity of present cropping practices, insofar as they are known. Yield increases considered attainable in these countries by *1960* are substantially equivalent to those considered attainable in the United States by *1950,* as reported in the U.S. Department of Agriculture's Miscellaneous Publication No. 593, "Peacetime Adjustments in Farming."

It is impossible with the data available to make predictions on possible production increases by individual crops the world over, because acreage, yield, and production of many important crops are not reported by all countries. Also, much of the world's food supply comes from animal products and the efficiency of this production in different countries is not known. But we can estimate probable increases in world production of the *eight* principal classes of food, by applying to each class the percentage increases of about the same magnitude of those thought attainable for principal crops in the United States by 1950. Thus, in Table II we have

the estimated world food supply possible in 1960 as a result of more intensive and better farming methods on present crop land, compared with the supply needed in 1960 to give all the people of the world an adequate diet, as estimated in the FAO survey.

In arriving at these estimates it is necessary to assume that the total prewar world food supply equaled prewar food consumption. Prewar world food supplies were, therefore, estimated by multiplying per capita consumption, as reported in the FAO survey, by world population. Larger percentage increases were applied to roots and tubers and fruits and vegetables because of the apparent opportunity for greatly increasing yields of these crops by the generous use of fertilizers.

On the basis of these estimates, world food needs in 1960 could be met for sugar and for roots and tubers on existing crop land. The need for cereals could virtually be met. Production of all other classes of food would fall short of the need.

It should be borne in mind, however, that the increases assumed to be attainable on existing crop land are conservative. There seems little doubt that a general use of high rates of fertilization on soils that will respond, coupled with modern techniques of insect and disease control, a change in land use patterns, selection of best varieties, flood and erosion control, and the adoption of other lesser techniques would result in even larger increases.

Bring new lands into cultivation:— There are even greater opportunities — but more difficulties perhaps — in increasing food production by bringing new lands into cultivation. At present only 7 to 10 percent of the total world land area is cultivated. Except for some desert areas, perpetual snow and ice, tundra, and the most rugged mountains, there is virtually no limit to the amount of land that can be brought into cultivation, save the economic limits of costs and returns. When we consider the Great Soil Groups of the world and the relatively small extent to which some of them are already in cultivation these possibilities become more apparent.

Soil maps have been made of many parts of the world. During the past few years our Bureau has made serious attempts to assemble these into a series of maps of uniform nomenclature so that various regions can be compared, and to fill in unmapped portions through careful study of climatic, geological, and other relevant data. This research is in progress. Although uncompleted, it has given soil scientists an opportunity to arrive at a few general preliminary judgments about our soil resources. A very small-scale, exceedingly generalized map of the world, compiled some time ago and shown on page 226, may be useful to indicate the location of the food soil areas of the world.

The snow and ice, tundra, mountains, and deserts total 48 percent of the world land areas. We can assume that these areas have no practical possibilities for extension of agriculture. The Chernozem, Chestnut, Gray Forest, Podzol (including Gray-Brown podzolic), the Red soils of tropics and subtropics, and Alluvial soils occupy an estimated 52 percent of the world land area. In this 52 percent we can look for areas for expansion of agriculture. The Chernozem and Chestnut soils are now largely under cultivation, and no great expansion into new areas can be foreseen. Some reclamation of Alluvial soils, either by drainage or irrigation or both, should be possible in the tropics.

PRASSOLOV (Pedology, No. 2, 1946) estimates the areas of the world in ten broad classes of soils. His estimates are as follows:

Chernozems Chernozem soils of prairies Black soils of tropical regions	6%
Chestnut soils of dry steppes (including Solonetz)	7%
Gray Forest Brown Forest Slightly leached soils of dry forests	7%
Alluvial soils, marshes, and swamps of tropical regions	4%
Podzols (including bogs)	9%
Red soils of subtropics Reddish-brown soils of tropical savannas Red soils of tropical forests Lateritic soils	19%
Sierozems and other soils of desert steppes and oases (including Solonchaks) Sands and stony soils of the deserts	17%
Mountain tundra Mountain meadows Mountain-forest Podzols Mountain-forest Brown soils Mountain-forest Red soils Soils of mountain steppes High mountain deserts	16%
Tundra	4%
Everlasting snow and ice	11%

The Podzols of the northern temperate zone and the Red soils of the tropics and subtropics constitute the extensive soils onto which great expansion of food production might be possible. These soils occupy an estimated 28 percent of the world land area, and probably less than 1 percent is now under cultivation. It is recognized, of course, that a large proportion of these Podzols and Red soils are unsuitable for agriculture because of the unfavorable topography and stoniness.

The principal areas of Red soils are in Africa, South America, Southeastern Asia, including India, the Pacific Islands, and Southeastern North America. Most areas of Red soils are now in use in Southeastern Asia and India, and large areas are in use in some of the Pacific Islands and in the United States; but the resources of these soils are almost untouched in Africa and South America. If we assume that only 20 percent of the Red soils of the tropics in South America and Africa alone were to be brought into production, about 900,000,000 acres would be added to the world acreage for food production. To these potential cultivated new areas of Red soils may be added a large area of uncultivated tropical soils found on the great islands of Sumatra, Borneo, New Guinea, and Madagascar. Assuming, then, that at least another 100,000,000 acres of Red tropical and Alluvial soils are available in these and other warm parts of the world, the total of one billion acres of tropical and subtropical soils may be used in calculating world soil potentialities.

The Podzols, located almost wholly in the north part of the northern hemisphere, are found mostly in Soviet Russia, Canada, and the United States. If we were to assume that only 10 percent of these soils were brought into cultivation, another 300,000,000 acres would be added to world acreage for food production.

The world's uncultivated areas are, of course, generally less fertile than those already under cultivation. To maintain continued productivity these soils would need fairly heavy fertilization. The question of serious erosion, especially in the tropical areas, needs to be met if there is expansion in this direction. In the last two decades, however, a body of science dealing with erosion control has been developed which gives us confidence that the problem could be met adequately. We could develop. We need not exploit.

If this huge body of 1,300,000,000 acres of new land were brought into production, what would be the world food picture? Again, without adequate production data we must estimate by making comparisons with production figures from similar areas. For estimating the possible production of the billion acres of tropical land we have chosen the Philippines as a yardstick because it is one of the few tropical countries that can be considered fairly representative of the tropical soil region and that had even approximately complete food production records. Finland similarly was chosen as representative of the 300,000,000 acres available in the Podzol group. By applying the approximate production per acre of the principal classes of food obtained in the Philippines to the billion acres of tropical soils, and that obtained in Finland to the 300,000,000 acres of Podzol soils, we arrive at the data presented in Table III.

World food needs in 1960 could thus be met for all classes of food except meat, milk, and pulses and nuts, by production increases on present crop land acreage plus the production of one billion acres of tropical soils, if these soils were used as intensively and for the same classes of food products as the cultivated soils of the Philippines are now used. We could have more than enough cereals, roots and tubers, sugar, fats and oils, and fruits and vegetables. In addition, the need for pulses and nuts could be easily met or exceeded by shifting some of the production of coconuts from oil to edible nuts.

If production of 300 million acres of North Temperate Zone soils were added to the increase from the present crop land and used as intensively and for the same classes of food products as the cultivated soils of Finland are used, the world food needs in 1960 would be met for cereals, roots and tubers, sugar, and milk. In addition, the need for fats and oils would be very nearly met.

World production from all our three sources combined — present world crop land, one billion acres of new tropical, and 300 million acres of extra-tropical soils — would exceed world food needs for all classes of food except milk and pulses and nuts, under the assumptions used, and would exceed them for all classes if a portion of the production of coconuts were shifted from coconut oil to edible nuts. Production of cereals, roots and tubers, sugar, and fats and oils would be far in excess of amounts needed.

To meet world food needs, then, much less than all of these sources of production are required, if efforts are made to produce primarily those classes of foods in deficit.

World fertilizer reserves adequate:— In discussing potential world food production either through intensified farming of old land or by developing new areas, there is one overlying factor affecting both. Greatly increased quantities of fertilizer will be required. The question arises, then — How adequate are world supplies of fertilizers?

Since nitrogen fertilizer can be manufactured by fixation of nitrogen from the atmosphere, world supplies are limited only by capacity of plants to produce. This plant capacity was expanded greatly in the last decade, because nitrates are a necessity of war.

TABLE IV: *Fertilizer in relation to expanded food production*[1]: —

RATE OF FERTILIZER USE IN KG PER HECTARE OF LAND USED FOR CROPS:

	P_2O_5	K_2O
World	5.0	3.2
United States	4.9	2.6
France	17.1	12.7
Hawaii	41.0	80.0

* * * * *

WORLD ANNUAL REQUIREMENT OF FERTILIZER UNDER EXPANDED FOOD PRODUCTION:

	P_2O_5 (*metric tons*)	K_2O (*metric tons*)
On 1,940,000,000 acres existing crop land at rate of France	13,425,000	9,970,000
On 1,000,000,000 acres (new) tropical soils at rate of Hawaii	16,400,000	32,000,000
On 300,000,000 new acres extra-tropical soils at rate of France	2,000,000	1,500,000
Total	31,825,000	43,470,000

* * * * *

	P_2O_5	K_2O
Known world reserves, billion metric tons	166	22.5
Years supply will last under expanded food production	5,200	500

[1] Data on fertilizer use from USDA Misc. Pub. 593.

With phosphate and potash, on the other hand, we must depend on natural deposits to fill world needs. In Table IV we compare known phosphate and potash supplies with potential needs for meeting world food goals. Again it is necessary to make several assumptions. First, we assume that present world crop land would scarcely need a heavier rate of applications than now used in France. You will note that the rate of fertilization in France is several times the present world rate. Second, we assume that the additional billion acres of tropical soils would scarcely need heavier applications than the rate in Hawaii, which is among the highest for all tropical areas. Third, we assume that the 300,000,000 acres of extra-tropical soils will again need no heavier rate of application than that of France. Thus, in all three areas we have chosen countries for comparative purposes which use fertilizer at a relatively high rate so that the estimate of adequacy on world reserves would be conservative.

The resulting amounts of fertilizer used under these conditions would be about 8 times the present consumption of phosphate, and nearly 18 times the present consumption of potash. Even so, the known world

reserves of phosphate would last more than 5,000 years and the known reserves of potash 500 years. The world has not been thoroughly explored for these minerals. Doubtless actual reserves exceed known reserves greatly. There are also many sources of potash, other than those included in the known reserves, that may be developed when economical methods of extraction are devised. This latter point again emphasizes how conservative these estimates are, for no allowance was made for technical improvements which are sure to come in the fertilizer industry.

Here, then, is an affirmative answer to the question: Do we have the natural resources to meet world food goals by 1960? This answer is a challenge to all men; not to scientists only. For it raises immediately an even more critical question: Can we mobilize these resources to produce the needed food? This question begs many answers, because it involves the whole field of human relationships.

Science may discover and point the way — but it cannot dictate. The full measure of success in economic, social, and political action comes only with the will of the majority — not from the desires of one group.

If the people of the world really have the determination to give battle to the problem of hunger; if they are willing to extend a small part of the energy and capital poured into World War II, only then can we see hope of victory.

To the Victors Belong the Opportunities

(courtesy of the Christian Science Monitor)

CROP PRODUCTION POTENTIALS IN RELATION TO FREEDOM FROM WANT

by K. S. QUISENBERRY

Head Agronomist, Division of Cereal Crops and Diseases, Bureau of Plant Industry, U. S. Dept. of Agriculture

IN AN address presented at the Annual Meeting of the American Society of Agronomy in Columbus, Ohio, in February 1946, Dr. P. V. CARDON (3)*, then Administrator, Agricultural Research Administration, U. S. Department of Agriculture, said: "We can, however, be certain at this time of a world-wide awakening to the paramount importance of food in the lives of all people, and to hope for continued peace that lies in the assurance of adequate food supplies. We have recently witnessed also the launching of the Food and Agriculture Organization of the United Nations upon which rests the burden of providing light and guidance to a hungry world groping to achieve freedom from want." For those who have lived in or traveled through the food and feed-producing areas of the United States during the past 5 years, and observed record-breaking crops in the fields, bulging granaries, long lines of loaded grain trucks, corn and wheat piled on the ground, mountains of baled hay, and lush pastures, it is hard to believe that many people of the world are hungry. But we know that such a condition does exist.

The question might be raised as to whether these bountiful harvests were the result of unusually favorable weather. To a certain extent they were since no critical weather conditions existed similar to the drought periods of the thirties, but in some cases growing conditions have been far from ideal. On the other hand, good cropping practices and the use of superior varieties had much to do with producing these large crops, and with establishing new yield records for corn, wheat, rice, and potatoes in 1946.

These improved cropping practices were based on research work dating back many years. The superior varieties were the result of plant-breeding efforts stimulated by the work of Mendel, and guided by the plant geneticists of the present century.

From plants we obtain much of our food. Of most importance are such crops as rice, wheat, corn, barley, oats, potatoes, sweet potatoes, soybeans, sorghum, millet, and possibly others. It is realized that from these crops alone a balanced diet is not possible and that it is desirable to have more variety. On the other hand, a diet supplying a minimum caloric requirement could be supplied and starvation prevented if an ample supply of these foods were available. Almost daily we read that certain countries of the world are in need and that food by the shipload is being dispatched to feed the hungry. It is safe to assume, therefore, that more food is needed if the world is to be free from want.

In order to show what some crop production potentials may be, it will be well to point out a few examples of what has been accomplished in the

* Figures in parenthesis refer to the references, p. 244/245.

United States by controlling hazards of production and increasing yields. With such a background, it will then be possible to indicate what could be done in other parts of the world, because after all it is desirable to produce a large share of the food not too far from where it is needed. As Dr. CARDON stated, the real need is to provide light and guidance.

Hybrid corn:— The story of hybrid corn in the United States has been told many times but it will bear repeating. It is no simple matter to determine exactly how much hybrid corn does contribute to increased production. M. A. MCCALL (6) in 1944 used a method of arriving at an estimated figure. Using this same method and through the kindness of MERLE T. JENKINS these estimates have been applied to the last six crops.

Each year for the period 1941 to 1946, inclusive, one-third or more of the corn acreage in each of the nine states of Ohio, Indiana, Illinois, Michigan, Wisconsin, Minnesota, Iowa, Missouri, and Nebraska was planted to hybrids. During this 6-year period these states produced 12,840,645,000 bushels of corn. The same acreage on the basis of the 1923-32 average yields (before hybrids were widely grown) would have produced 9,409,889,000 bushels, so there was an increase of 3,430,756,000 bushels. Attributing this increase to the use of hybrids, each of the 255,370,000 acres of hybrid corn grown in these nine states during the 6 years gave an average increased annual yield of 13.4 bushels an acre.

Since the 1920's a large increase in the use of commercial fertilizers in corn production and marked gains in combatting disease and insect pests have occurred. Consequently not all of the increase in acre yields in the 1940's over those of the earlier period are attributable to the introduction of hybrids. Furthermore, the general use of mechanized equipment in recent years has enabled growers to plant the corn crop more nearly at the optimum time of year than was possible with horse powered equipment which was more generally used in the earlier period. Nevertheless, the change-over to hybrid corn is without doubt the principal cause of the gains in acre yields in the later period. The majority of the hybrids which have come into general use have greater capacity for utilizing the plant nutrient materials in soils and in fertilizers for the production of grain yields than open pollinated varieties; and they likewise have more ability to withstand insects, diseases and adverse weather conditions.

These estimates may be applied to a single year. For example, in 1946 in the nine states listed, the hybrid corn acreage was estimated at 49,095,000. Using the 6-year average increase per acre of 13.4 bushels from growing hybrids, the total increased yield in 1946 was 657,873,000 bushels. Assuming only one-half as great an increase (6.7 bushels) on the remaining 13,585,000 acres of hybrid corn grown in the United States in 1946 an additional 91,020,000 bushels may be added, giving a total of 748,893,000 bushels increase in yield due to the use of hybrids. This is an astounding figure, but from careful calculations it seems reasonable and conservative. These figures help explain how it has been possible to produce five successive corn crops each exceeding 3 billion bushels on annual acreages of between 91 to 97 million. The 1946 corn crop was 217 million bushels larger than the one produced in 1920, and it was grown on approximately 10 million fewer acres. This is a most remarkable accomplishment.

Wheat improvement:— The United States now produces about 425,000,000 more bushels of wheat than 50 years ago, an increase of 70 percent. This increase is mostly due to growing more acres, since on the whole the average yield per acre has increased very little. Actually, it is only 2.1 bushels per acre based on the two 10-year periods ending in 1895 and 1945, or 1.1 bushels per acre if 20-year periods ending in the same years are compared. This small increase confuses many people who seek in larger yields per acre support for their belief that there have been marked improvements in varieties and production methods, and when such figures are not found, are sometimes inclined to doubt whether any improvements actually have been made. They overlook the fact that the greater acreage is itself very largely, if not entirely, due to improvements in varieties and methods; also that when the acreage is increased by bringing less productive land into cultivation as has usually been the case in this country, even maintaining the average yields means that larger yields are being produced in the more productive areas. There is also a tendency for disease and insect pests to multiply as the acreage of a given crop increases.

It may be well to consider some of the reasons why we are now able to grow practically a billion bushels of wheat per year. The only varieties of wheat grown in the northern Great Plains 50 years ago were the late-maturing Haynes Bluestem, Powers, and Red Fife. They were and are good varieties when conditions are favorable, *i.e.*, plenty of moisture and no rust. But rainfall in that area is seldom plentiful and, beginning soon after the turn of the century, black stem rust epidemics seem to have occurred with greater frequency, and more devastating results. Marquis wheat, first introduced in 1912, was hailed as a godsend and increased so rapidly as to nearly replace all other varieties by 1919. It matures early and thereby escapes some of the damage caused by stem rust, drought and heat. The Ceres variety, slightly earlier and resistant to some races of rust, was introduced in 1924 and resulted in much additional improvement. Both Ceres and Marquis fell by the wayside in the great stem rust epidemics of 1935, 1937, and 1941. In the meantime Thatcher had been released to farmers and, like Marquis 25 years earlier, increased by 1941 to the unprecedented total for a single variety of approximately 17 million acres in the United States and Canada. But Thatcher, like its predecessors, was not good enough, for although resistant to stem rust it was susceptible to leaf rust, and while this disease is less destructive than stem rust it does cause damage. Again plant breeders were ready, this time with such varieties as Rival, Pilot, Renown, Regent, Mida, and Cadet, and these have practically replaced Thatcher in the rust areas of Minnesota and the eastern Dakotas.

Farther south on the Southern Plains, the same story is unfolding, though less spectacularly. The hard winter wheat region of this section built its wheat industry around Turkey introduced from Russia in about 1873. It was regarded as a satisfactory variety until the first or second decade of the present century. It was sometimes seriously injured by drought and occasionally by rust, but who would be so visionary as to suppose anyone could do anything about drought, and stem rust occurred so infrequently that it could be ignored, while leaf rust was supposed to be more or less harmless. Some were not willing to believe that because Turkey was a good variety that it was necessarily the best, so plant breeders

began to produce earlier varieties to escape the destructive effects of hot winds and avoid some of the damage from rusts. These were not entirely satisfactory so the desirable qualities of Turkey were combined with resistance to rust, to the smuts, and in some cases to hessian fly. From such work have come the varieties Tenmarq, Pawnee, Comanche, Wichita, Westar, and others that in many sections of the Southern Plains have completely replaced Turkey.

Two principal lines of attack have been used in the control of stem rust of wheat: barberry eradication and breeding for resistance. The barberry serves as the alternate host of the fungus and, since the sexual stage occurs on this plant, its elimination should reduce the possibilities of the production of new physiological races of the organism. Also the barberry may be responsible for supplying inoculum early in the season and in isolated areas; thus the eradication campaign has contributed to rust control, especially in local areas.

In addition to producing wheat varieties resistant to stem rust and adapted to local conditions, the breeding work has gone still further. It is recognized that national epidemics of stem rust on hard wheats are caused by spores blown from Texas north through the Plains to the Dakotas and Minnesota. Then in the fall spores are blown south where they may cause infection and overwinter, ready to provide more spores for the northward movement the next year. It was realized that if the build-up of these spores in Texas could be stopped, the chances of serious epidemics would be lessened. Again the plant breeders went to work and with the help of the plant pathologists produced the variety Austin that is resistant to leaf and stem rust and is well adapted to central Texas. This wheat is spreading rapidly and its use should greatly reduce the amount of stem rust inoculum usually available in Texas in the spring. Add to this the fact that across Oklahoma, Kansas, and Nebraska, varieties with some resistance are being grown, so that the spores from Texas are not so apt to increase and be blown on north into the Dakotas and Canada. With the resistant spring wheats the chances of the spores increasing in the North are reduced and as a result there is less inoculum to blow back South in the fall to start the cycle all over again if the weather is favorable. Here is an example of varieties in one location influencing conditions a thousand miles away.

It has been estimated that new varieties produced by the experiment stations and grown by farmers are responsible for an increased production amounting to about 170,000,000 bushels per year above what would have been possible had farmers depended on varieties available 25 years ago. In addition, the crop that is harvested is of better quality and of higher market value.

Oat breeding:— A significant change in oats has been, and is, taking place in this country with the development of varieties resistant to crown and stem rust and to smut. A number of new varieties have been released such as Tama, Vicland, Cedar, Vikota, and Clinton, to name only a few. These varieties are characterized by strong straw and, together with rust resistance, are able to stand well for combine harvesting. In addition, they have delayed germination so as to resist sprouting in wet harvest seasons. All have large, heavy kernels giving high test weights per bushel and thus are better for feed and food. Yield increases range from 10 to 30 percent in experimental plots.

It has been stated that in Wisconsin the years 1942 and 1945 were quite similar so far as climatic conditions for oat production were concerned. In 1942 the average yield in the state was 43.0 bushels per acre when only 90,000 acres of Vicland were grown. By 1945 Vicland had increased to nearly 3 million acres and the state average was 51.0 bushels per acre.

In the United States in 1945 there were 41,503,000 acres of oats of which at least 25 million were sown to improved varieties. The average yield in 1945 was 37.3 bushels as compared with 31.1 bushels per acre in 1941. This increase was due for the most part to the use of improved varieties. If an increase of only 4 bushels per acre is assumed for the 25,000,000 acres of new varieties, an increase in production of 100,000,000 bushels is obtained for a single year. Conservative estimates suggest that since the new varieties have been in use they have made possible the production of 500,000,000 extra bushels of oats.

Rice:— Rice is one of the most important food crops in the world, although of relatively less importance in the United States. In this country, however, improved varieties and better cultural, irrigation, and fertilizer practices have given most outstanding results. Some average yields for the 5-year periods ending in 1908 and 1943 may be compared as follows:

Place	Average yield in bushels for 5-year period ending	
	1908	1943
Arkansas	39	50
Louisiana	29	40
Texas	33	48
United States	32	46

Between these two periods, 35 years, average yields increased from 27 to 43 percent. In California alone the increase from 1913 to 1943, just 30 years, was 42 percent, with a State average of 67 bushels per acre in 1943. These figures are almost unbelievable yet they show what applied research might accomplish with this crop on a world basis.

Potatoes:— A well-planned, cooperative improvement program in potato breeding has been in operation for 16 years in the United States, and numerous new varieties have been released. In the United States the average yield of potatoes in 1917 was 100 bushels per acre, while in 1943 it was a little more than 139 bushels per acre, and in 1946 the yield was approximately 184 bushels per acre. According to STEVENSON (7) the factors contributing to this increase are new disease-resistant varieties, more efficient disease and insect controls, certified seed, and improved fertilizer and cultural practices. Also, a trend to increase the acreages on the more fertile soils of Maine, Idaho, and California has been a factor in increasing the average yield per acre.

In the last few years experiments with new chemicals to control aphids on potatoes have shown much promise. At Houlton, Maine, in 1945, BRONSON and SMITH (2) found that the addition of DDT to the fungicide spray used increased yields from 370 to 547 bushels per acre or 48 percent,

while at Beltsville, Maryland, in 1946, FOSTER* reported that a 5 percent DDT clay dust increased yields from 301 to 390 bushels per acre. Both of these were single year tests but they are very suggestive of what may be expected in the future. Improvements in all lines continue and it is not too much to expect that before many years the yield per acre will reach an average of 200 bushels.

Improvement of forage crops:— Improvements have been made with forage as well as with grain crops. If large numbers of livestock are to be maintained then adequate forage must be supplied. As shown by HEIN (4) crested wheatgrass is an outstanding example of a plant introduction that has made history in this country. Introduced from Russia in 1898, and again in 1906, this grass proved to be well adapted to the northern Great Plains. Following the drought years of the early thirties it was realized that a good grass was needed to re-establish a useful crop on the vast areas plowed up for cereal production. Crested wheatgrass proved to be the crop to use for pasture, hay, and ground cover to prevent wind erosion. This plant starts growth early in the spring, and is ready for grazing 3 to 4 weeks before the native range grasses. As a hay crop it has consistently outyielded its chief competitors, bromegrass and native wheatgrass. It is best adapted to the drier areas of the northern plains of the United States and Canada. In 1944 it was established on 3 million acres of abandoned grain fields alone, and it has spread rapidly. Here is a crop-producing feed in areas where feed is often very scarce.

Alfalfa:— Two new bacterial wilt-resistant alfalfa varieties, Buffalo and Ranger, have been developed in coöperative breeding programs between Kansas, Nebraska, and the U. S. Department of Agriculture. In three states, Ohio, Minnesota, and Iowa, the average superiority of Ranger over old varieties has been 1.69 tons per acre in the third crop year. In the 12 North Central States there are approximately 7½ million acres of alfalfa of which at least half is subject to the bacterial wilt disease. Assuming only one year out of an average four-year rotation, and a yield advantage of 1.69 tons per acre, the average increase in production would be approximately 1,584,375 tons annually if this acreage were planted to superior varieties.

The 11 Western States have a total 4,931,000 acres in alfalfa, much of which is under irrigation. Yield data indicate an increase of as much as 3.40 tons per acre if improved varieties are used where disease is a serious factor. To be conservative again assume an increase of 1.69 tons per acre on half of the acreage and in the fourth crop year and the increased production would amount to one million tons annually. This means, according to TYSDAL (8, 9), that the use of improved varieties from Ohio westward would be expected to increase the average production by more than 2½ million tons of alfalfa annually. The total production of alfalfa hay in this area is about 28 million tons per year, so the increase possible amounts to approximately 9 percent. It is safe to predict that breeding programs will develop strains more superior under Eastern conditions than Ranger and Buffalo are under western conditions, and strains superior to Ranger and Buffalo are in the offing.

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* Unpublished data.

Red clover:— In the northern and central red clover areas there is a serious disease known as northern anthracnose. Midland red clover is well adapted to the area and has some resistance to this disease as shown by HOLLOWELL (5). Depending upon the environmental conditions increased yields from one quarter to three quarters of a ton per acre can be expected of Midland over common red clover. For the southern part of the area Cumberland is well adapted and it has some resistance to southern anthracnose and some resistance to crown rot as well. Cumberland frequently yields a ton per acre more than unadapted red clover in the southern red clover belt.

Only a few examples have been given of outstanding accomplishments of crop improvement in the United States. Other varieties could be discussed as the work has included grain, forage, fruit and vegetable crops, and in each case the results have been most encouraging.

Are results such as these possible in other countries of the world?

Crop improvement in the world:— According to ÅBERG (1), yields per acre of various crops in Sweden have increased since 1871-75 as follows:

Crop	Increase in bushels	Percent increase
Winter wheat	19 to 38	100
Spring wheat	18 to 27	50
Rye	18 to 27	50
Potatoes	106 to 180	70
Barley	19 to 32	68
Oats	17 to 27	59

These increases are attributed to better drainage, use of fertilizers, and plant breeding.

A study of Japanese agricultural statistics made by Dr. S. C. SALMON has shown that yields have increased as follows, the comparisons being based on average yields for 5-year periods ending in 1882 and 1942:

Crop	Increase in bushels	Percent increase
Rice	43.0 to 73.0	71
Wheat	11.6 to 27.8	140
Common barley	17.7 to 38.8	119
Naked barley	18.1 to 29.3	62

These increased yields are attributed to the use of commercial fertilizers and better varieties produced by breeding. Here are examples of sizeable increases in other countries showing clearly that the United States is not alone in crop improvement work.

Consider, for a moment, the possibilities of crop improvement in countries where food is known to be scarce. In China and India, rice is of paramount importance as a food crop, and yield increases would do much good. In China the average yield per acre of rice is reported to be 53 bushels while in India it is 26 bushels. Mr. JENKIN W. JONES has stated that there is an excellent opportunity to materially increase the acre yields

of rice in monsoon Asia by the breeding and distribution of improved varieties and by the use of better cultural, fertilizer, and irrigation practices. Assume that the rice yields of China and India could be increased by only 10 percent, and in light of what the United States and Japan have done, this is not unreasonable. Such an increase would add 255 million bushels in China and 206 million bushels in India, or a total of 461 million bushels annually. It has been estimated that among rice-eating people the average per capita consumption varies from 150 to 400 pounds per year. If we assume an average of 250 pounds per year, the increased production mentioned above would supply food for 58,086,000 people.

Equally important increases could be postulated for other countries of the world. It is easy to see what great benefits would occur if wheat production in China, India, Italy, and France could be increased as in our own country. Also the benefit to Germany, Poland, and some other countries would be tremendous if the yields of potatoes were raised by 60 percent.

It is realized that the problem of providing more food by improved cropping practices is not as simple as outlined above. In many cases the improved varieties have not been produced, and if produced the question of adequate seed supplies must be answered. Also many people are so impoverished that they could not obtain this seed unless it came as a gift. The next barrier is getting the story to the people, many of whom cannot even read, and having them accept it. This will be a real difficulty, judging by the experiences often encountered in as progressive a country as our own, especially if the first recommendations should be slightly in error. As Dr. CARDON has pointed out, many people of the world may be bound by custom, tradition, myth, or superstition, and would be reluctant to believe that plant diseases could be controlled by resistant varieties, or seed treatment, or that disease control in one country might influence crops in another. Add to this the lack of transportation and storage facilities, and the task appears more difficult. We must be neither too optimistic nor too discouraged about the task ahead, one that will take years to complete as well as the whole-hearted efforts of many workers. People are less isolated and are becoming better acquainted, so the day may dawn when crop-improvement problems of the world will be attacked by the cooperative effort of several nations, just as states of the United States now work together in coordinated programs. Under such a plan, improvements should be more rapid and more widespread, thus providing food to keep more people free from want.

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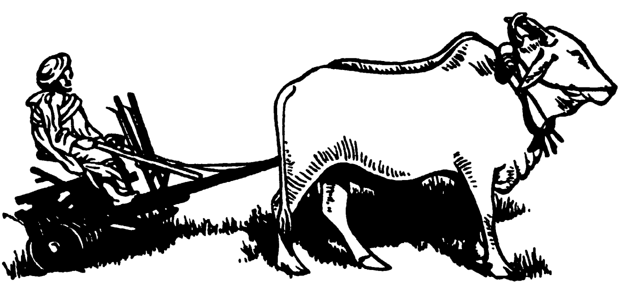

INVITATION TO ANARCHY

(courtesy of St. Louis Post Dispatch)

ANIMAL PRODUCTION IN AN EFFICIENT FOOD ECONOMY

by F. B. MORRISON

Professor of Animal Husbandry and Animal Nutrition, Cornell University

IT IS well known that in areas of the world where the density of population presses most upon the food supply, foods of animal origin form but a small part of the total food consumed by humans. In such areas, humans must eat all or nearly all the edible food produced on arable fields, and farm animals can have only the forage or other feed which cannot be consumed by humans. This is because there is a large loss of heat or energy value, measured in calories, in the conversion into animal products, of grain or other feed eaten by animals.

Therefore in such parts of the world, relatively few farm animals can be kept in cultivated areas for the sole or primary purpose of producing food for humans. Animals may be kept as a chief source of draft or work, perhaps also producing a very limited amount of milk for humans, and to be eaten for food when they are no longer efficient work animals. Areas that are too rough, too arid, or otherwise unsuited for crop production, are grazed by sheep, goats, or cattle, which contribute to the small supply of foods of animal origin. Swine and poultry are kept chiefly as scavengers, living mainly upon garbage and other waste products.

Proportion of animal foods in the human diet:— According to recent estimates by PEARSON and HARPER (1945) grain and grain products make up about 73 percent on the dry basis, of the total food consumed by the world's population. Vegetables and fruits form 12 percent, sugar 6 percent, and all animal products about 9 percent. Included in the animal products are milk and milk products, estimated at 5 percent on the dry basis, meat at 4 percent, and fish and eggs each at less than one-half percent.

The proportion of the total human food which comes from animal sources differs greatly in various regions. According to PEARSON and HARPER, in Asia foods of animal origin form only 3 percent of the human diet, on the dry matter basis, and in Africa only 4 percent. In South America the percentage of foods of animal origin is 16, in Europe 17, and in North America 25 percent. Oceania (over three-fourths of whose population is in Australia and New Zealand) consumes a diet with the largest proportion of food of animal origin, 36 percent.

These great differences in the proportion of animal foods in the human diet in the various regions of the world naturally raise questions concerning the desirable goals in considering the problem of feeding the world's population adequately in future years. Can the proportion of food of animal origin possibly be increased to the North American level throughout the world? On the other hand, should we all live at Asiatic levels, with only 3 percent of our food from animal sources, on the dry basis?

PEARSON and HARPER estimate that if the world would accept the Asiatic standard, our present grain production would support over 2,800,000,000

people, instead of the present number of 2,170,000,000. On the other hand, under a North American standard of living, the present world production of grain would maintain only about 900,000,000 persons, or less than half of the present population.

Relative efficiency of various farm animals on basis of energy:— Such data lead naturally to a consideration of the relative efficiency of the various farm animals in the production of human food and to a discussion of the special values in the human diet of foods of animal origin.

Let us first consider the efficiency with which various animals convert the gross energy of the feed they consume into the energy of animal products eaten by humans. All will agree that this is an inadequate measure of the efficiency of farm animals in food production, because we do not eat animal products primarily as a source of energy. We can get energy at a fraction of the cost by eating grain products or such foods as potatoes.

We need not review here the various estimates that have been made by different investigators of the relative efficiency of farm animals in human food production. These have agreed fairly well in general conclusions, but have shown some decided differences.

MAYNARD (1946) has recently presented the following estimates of the relative efficiency of the various farm animals in converting the gross energy of their feed, measured in calories, into the gross energy of edible products consumed by humans. He states that these estimates are based upon studies of feeding experiments and other available data.

The estimates are based on somewhat better than average levels of production, obtained under good feeding practice. For milk production the estimates are based on an annual yield per cow of 7,000 to 8,000 lbs., which is considerably above the United States average. The estimates for pork are for the period from weaning to slaughter at approximately 230 lbs. live weight.

The estimates for beef and lamb are limited to the fattening period in the feed lot, because of the impossibility of measuring the feed intake from pasture or range. The data for beef production are for calves started on feed at a weight of about 450 lbs. and carried to a market weight of 850 to 1,200 lbs. The estimates for lambs are for lambs fed from a weight of approximately 65 lbs. to a market weight of about 90 lbs. For poultry meat the estimates cover the period from hatching to market, and for egg production the figures are for an annual production level of about 150 eggs per bird.

In these estimates and also in others which have been made, pork production and milk production rank far ahead of other types of animal production, when measured by the percentage of the gross energy of the feed which is recovered as edible human food. The percentage recovery in pork production is estimated at 15 to 25 percent, with an average of 20 percent. For milk production the estimated range is 12 to 18 percent and the average 15 percent.

In comparing these two types of animal production, we must bear in mind that the hog, at least under conditions such as in the United States, lives chiefly on grain and other concentrated feeds which are high in digestibility and consequently high in net energy value. On the other hand, only about one-third of the dry weight of the feed of dairy cows usually consists of grain and other concentrates. Two-thirds comes from pasturage, hay,

silage, and other roughages, all of which are much lower in digestibility and in net energy value than the grains.

Measured on the basis of the efficiency with which they convert either digestible nutrients or net energy into human food, dairy cows would rank somewhat above swine. Also, measured on the basis of the calories of human food produced per acre of crop fed, milk production would rank slightly above pork production.

It must be borne in mind that these estimates of the efficiency of milk production are based upon the human consumption of all the food value in milk. When butter is eaten and the skimmilk is used for animal feeding, the efficiency of human food production is, of course, decidedly lower.

Other animals rank much below pigs and dairy cows in the percentage of the gross energy of their feed which they convert into edible human food. MAYNARD estimates the percentage recovery as 7 percent in egg production, 5 percent in the production of poultry meat, and 4 percent for the production of beef or lamb.

In all such comparisons certain facts must always be borne in mind. Swine and poultry are direct competitors with humans for food, because they live chiefly on grain. Dairy cows compete with humans for food to a much greater extent, for this country as a whole, than do beef cattle or sheep. This is because our dairying must be located almost exclusively on arable land which could be used for the direct production of crops for human food. While dairy cows on a considerable proportion of our farms utilize pasturage on some areas which could not readily be tilled, such forage forms but a small part of the total feed eaten by our dairy cows.

The great majority of the beef cattle and sheep in the United States is raised on the western ranges or on other grazing land that is unsuited to tillage under present conditions. Here they convert forage that would otherwise be wasted into valuable human food and other products needed by man, such as wool and hides. Except for the very limited amounts of harvested roughage and of concentrates which may be needed during the winter period, range beef cattle, sheep, and Angora goats do not compete with humans for food, or even with dairy cows, swine, and poultry.

In his studies on "Relation between feed, livestock, and food at the national level," JENNINGS (1943) has estimated the number of "feed units" required by the various farm animals to produce 2600 calories of human food, or a day's allowance for an average person.

These feed units are approximate estimates of the values of various feeds, with 1 lb. of corn grain being taken as unity. Hay and other roughages are given lower values, approximately in proportion to their net energy values. Protein supplements, such as soybean oil meal, are given a considerably higher value than corn grain, although their net energy values tend to be lower, rather than higher, than that of corn. In his estimates, for example, soybean oil meal is given a value 60 percent higher than that of corn.

Measured on this basis, JENNINGS estimates that for the production of 2600 calories in milk, dairy cows require 9.31 feed units, but 3.69 come from pasture, leaving only 5.62 feed units from harvested crops. In comparison, 7.17 feed units in addition to pasture are required to produce 2600 calories in pork.

Sheep get such a large part of their feed from pasture that although

74.47 total feed units are required to produce 2600 calories in lamb, 60.64 come from pasture, leaving only 13.83 to come from harvested feeds.

Relative efficiency of various farm animals considering other factors:— Foods of animal origin have a much higher value because of their supplies of protein, of minerals (especially calcium and phosphorus), and of vitamins, than because of the energy they provide. It is difficult to take all these nutrients into definite consideration in comparing the economy of various food sources. However, protein can and should certainly be considered, along with calories. Not only are the grains and most other foods of plant origin low in protein content, but also their protein is generally of poor quality, containing insufficient supplies of certain of the essential amino acids. Milk, meat, and eggs make good these deficiencies in protein in a very efficient manner, for they are rich in the very amino acids that are otherwise apt to be deficient in the human diet.

To take both energy and protein into consideration, JENNINGS has estimated the feed units required by the various farm animals to produce what he calls "a calorie plus protein index." In this index the production of 0.15 lb. of protein (a day's allowance for an average person) is given the same weight as the production of 2600 calories.

Measured on this basis, he estimates that the following amounts of feed units are required for the production of a calorie plus protein index of 1.0: milk production, 6.5; pork production, 9.2; turkey meat, 9.7; eggs, 10.7; chicken meat, 12.0; beef, 41.3; and lamb, 51.7. These estimates are for average conditions in the entire country.

Note especially that on this basis of comparison, JENNINGS rates turkeys nearly equal to swine in efficiency of meat production and even ahead of egg production by chickens.

Considering total feed consumed, beef cattle and sheep fall far below the other farm animals in efficiency. However, JENNINGS also gives similar estimates of the grain and other concentrates, not including pasture, hay, and other roughage, required by the various farm animals to produce a calorie plus protein index of 1.0.

On this basis, dairy cows lead, with a requirement of only 1.6 feed units. They are followed by sheep, which need only 3.2 feed units from grain and other concentrates. Swine, beef cattle, and turkeys all require about 8.7 feed units. Chickens need 10.1 feed units for egg production and 10.9 for meat production.

Milk production not only ranks first in efficiency of production when both energy and protein are considered, but milk also is of especial value in the diet as a source of calcium and vitamin A. Both of these nutrients are needed in especially large amounts by children and by pregnant and nursing mothers. Milk is also rich in phosphorus, but a deficiency of phosphorus occurs in human diets much less frequently than a lack of calcium or of vitamin A.

MAYNARD emphasizes that since calcium cannot be supplied adequately by any combination of vegetable foods which our people can be expected to eat, the high content of calcium in milk is of especial importance. One quart of milk a day supplies the full allowance of calcium recommended by the Food and Nutrition Board for a growing child, except during adolescence, and more than is needed by the adult, except for expectant and

nursing mothers. It would take 45 eggs to supply as much calcium as is furnished by a quart of milk.

The importance of milk and of all dairy products which include the butterfat as sources of vitamin A in the human diet is so generally recognized that it needs no discussion. Milk is also rich in riboflavin.

The discovery of a new unidentified factor or vitamin, called X, in milk and certain other foods and feeds has recently been announced by HARTMAN and CARY (1946). This discovery seems to give another reason for advising an adequate proportion of milk in the human diet.

When young rats depleted of this factor are fed diets deficient in it, but adequate in all known nutrients, their growth and development are decidedly retarded. The factor occurs in milk, skimmilk, dried skimmilk, cheese, egg yolk, beef and pork muscle, bluegrass, alfalfa hay, timothy hay, and lettuce. It is not present in the grains studied thus far, or in wheat flour, soybean oil meal, linseed oil meal, egg white, or yeast. A concentrate has been prepared which is so potent in the factor that only two micrograms per day increases the growth of a young rat appreciably.

While it is too early to evaluate the importance of this discovery in human nutrition or in livestock feeding, the findings seem to have great significance.

Eggs are important sources of vitamin A value, being three times as rich as milk on a calorie basis. They are also an important source of calcium, are the richest of all animal feeds in iron, are high in riboflavin, and are the only important source of vitamin D among natural foods.

In addition to its high value as a source of excellent quality protein, meat is important in furnishing iron and certain B complex vitamins, especially niacin. Pork ranks high in thiamine content, and it is estimated that nearly half of the thiamine in the average diet in this country is supplied by animal products.

An important point is that ruminants do not apparently require the B complex vitamins in their feed, because these vitamins are synthesized in the fermentations which occur normally in the rumen, or paunch. On the other hand, pigs and chickens, like humans, must have an adequate supply in their food.

An exceedingly important quality of most foods of animal origin is their high palatability. Not only do they taste good in themselves, but they also greatly increase the acceptability of other foods. I believe that any of you who have experienced the monotony of the efficiently rationed British diet during the war years will agree with me that it was a great pleasure to get back to our American diet.

The universal desire of our people for a liberal amount of meat, when they can afford it, was dramatically shown this summer, when meat supplies became so scanty, and long queues and great dissatisfaction and complaint resulted.

Considering all factors, milk production easily ranks first in importance and efficiency of production, among foods of animal origin. For this reason, every effort was made in Great Britain and other European countries during the war years to keep milk production as near the normal level as possible, and to restrict severely the concentrated feeds allotted for other classes of farm animals.

From the energy standpoint, pigs would rank next, but considering all

factors, second place in efficiency should probably be given to egg production.

From the standpoint of national food economy, the production of beef and lamb also ranks very high, insofar as the meat is produced largely from forage which would not otherwise be utilized.

Other reasons for ample foods of animal origin:— In considering the place of animal products in our economy, we must bear in mind that farm animals furnish other products. The farm manure they produce is highly important in the maintenance of the fertility of our farms. Other important products, of course, are wool, mohair, hides, inedible products for animal feed, and an array of special packing house by-products.

Babcock (1946) emphasizes that there should be a liberal amount of foods of animal origin in our diet for reasons entirely apart from nutritional. He points out that farm animals are an effective means of condensing and storing a surplus of feed in a period of great plenty. The numbers of meat animals then increase and a reserve is built up which is drawn upon for meat in a following leaner period. Farm animals are thus a reserve food supply for time of drought or war.

Another important point is that any pronounced reduction in animal products would greatly reduce the number of family farms. Crop production can be highly mechanized and can be conducted with great efficiency on a large scale, in strong contrast to most types of livestock farming.

The processing and distribution of animal products provide much more employment and industrial development than would result if we lived nearly entirely on cereals and other foods of plant origin.

Babcock predicts that in this country, instead of using less animal food in the future, our people will use more because of two important factors: First, more education on an adequate diet; and second, the rapid and great development which is taking place in quick freezing and low temperature storage of food. He concludes that if our 140,000,000 people all ate the kind of diet that is best for them, they would consume decidedly more food than can at present be produced in this country. We would have no agricultural surpluses to worry about.

Possible increases in production of animal products:— In considering the place of animal products in our national economy, we must give attention to the possibilities of increasing the supply of these products as our population grows, without decreasing the yield of crops grown for human food.

First of all, the amount and the efficiency of animal production could be very considerably increased if all our farmers adopted and carried out the methods now used by the most efficient third. For example, the average milk yield per cow could be increased from the present national average of less than 5000 lbs. to the average in Dairy Herd Improvement Association herds throughout the country, which was 8592 lbs. in 1945. All that is needed for such an increase is the putting into practice by all farmers of the methods and practices that are now well known.

To reach this goal, cows must, first of all, be provided with a more abundant supply of roughage of higher quality. This means better pasture, such as that furnished by combinations including such efficient and nutritious pasture crops as Ladino clover, alfalfa, bromegrass, and lespedeza. It also means the production of better hay, curing it efficiently, and feeding

it in abundance. In most dairy districts, it will moreover mean the providing of more high-quality silage from corn, the sorghums, or from hay crops, ensiled by special methods.

By supplying cows with an abundance of such roughage throughout the year, the amounts of grain and other concentrates required to maintain high milk production can be reduced decidedly. Yet it is essential that good dairy cows be fed sufficient concentrates for a high yield under our usual conditions. When they are fed roughage alone, even of excellent quality, the milk yield is greatly reduced.

The amounts of good roughage produced on most of our dairy farms can be greatly increased on the present acreage by more use of fertilizers, combined with well-planned crop rotations and with soil-conservation methods. Not only will the amount of pasture, hay and silage thus be increased very largely, but also and fully as important, the quality will be much improved, because of a greater proportion of legumes in the forage.

Such a program of abundant and better roughage for dairy herds should increase, rather than decrease, the supply of grain for human consumption. The fields used in rotation for grain production, with legumes in the rotation, will produce much higher yields than under less efficient cropping methods. Actually, a smaller acreage per cow may be needed to supply ample high-quality pasture and harvested forage than is now needed to furnish a rather scanty amount of mediocre roughage.

It is important to note that the improved roughage feeding of dairy cows which will increase their production, will also increase the vitamin A value of milk and butter markedly. In particular, an abundance of high-quality hay and especially of hay-crop silage will maintain a high vitamin A value in the milk in winter, when otherwise it has a lower amount.

The rapid development of artificial insemination of dairy cows is making possible a decided increase in the productive capacity of our cows. Through this means a dairyman can at moderate cost have his cows bred to a bred-for-production sire of a transmitting ability that has not hitherto been generally available to most farmers.

Whether hormones, such as thyroprotein, will have any important place in increasing our dairy production cannot be decided as yet. Likewise, the place of cross breeding in dairying is a hotly disputed question.

As our population increases and we need more food, the production of beef and lamb can be considerably increased by converting some of our semi-abandoned hill lands into first-class pastures. There are considerable such areas in the northeastern states and in many sections of the South. Large areas of the cut-over lands of our northern states are also yet undeveloped.

Excessive fattening of beef cattle wastes feed:— Meat is produced most efficiently when the animals are not carried to an excessive degree of fatness. As the fattening process progresses, and especially after the animals have become fairly fat, the amount of feed required for each 100 lbs. of gain in liveweight rises steadily.

This is because the gain consists to an ever-increasing extent of a higher proportion of fat, which has a much higher energy content than protein. Also, the tissues gained in the latter stages of fattening have much less water. Another reason for the higher feed cost of gains when animals are already well fleshed is that they then consume less feed per 100 lbs. live-

weight and have a smaller amount of nutrients left for storage, after the maintenance requirements of the body have been met.

Carrying meat animals to a high degree of fatness is especially wasteful in the case of cattle and sheep, because most of us do not eat all of the large masses of fat in highly-finished beef or mutton, no matter how appetizing the lean meat is.

Young cattle fattened to the carcass grade of Good make palatable beef that will meet the desires of most consumers, providing that the carcasses are properly aged. NELSON (1945) has calculated that of the 115 billion pounds of gain produced annually on cattle fattened in our Corn Belt, about 80 percent was made before or by the time they reached an average slaughter grade of Good. To produce the 20 percent of the gain made after this degree of fatness was reached required 30 percent of the grain and protein supplement eaten by the cattle.

He points out that a relatively large part of this latter gain was probably not consumed as human food, because it was largely fat in excess of what the average person consumes along with lean meat. He estimates that of the 300,000,000 lbs. of gain produced after the Good slaughter grade was reached, only 60,000,000 lbs. was probably consumed as human food. He estimates also that if the corn and protein supplement which are normally used in the Corn Belt in a year to fatten cattle beyond the Good grade were used to fatten additional cattle, about 1,700,000 more animals could be fattened. FRANCIS, BULL, and CARROLL (1944) have also recently emphasized the wastefulness of carrying cattle to an excessive degree of fatness.

Pork production efficient to market weight of 300 pounds:— In pork production the situation is much different from these conditions in the fattening of cattle. As in the case of beef cattle, the proportion of fat in the carcass increases as growth and fattening progress, and the proportion of lean decreases. However, the packers largely standardize such pork cuts as hams, shoulders, and loins by trimming off excess fat, which is then used for lard or for fat backs in the case of loins.

Another important point is that the average person eats a much larger proportion of the fat in a well-prepared pork cut, even though it is rather fat, than he does in the case of fat beef.

The type of pig common in this country a generation ago, the old-fashioned lard-type, became excessively fat at weights over 200 lbs. However, the present type, called the intermediate type, is much more growthy and does not become excessively fat until the pigs weigh more than 300 lbs. While the amount of feed required per 100 lbs. gain by the old lard-type pigs increased rather rapidly after they reached a weight of 200 to 225 lbs., this is not true with the modern type.

ATKINSON and KLEIN (1945; 1946) have recently published excellent studies on feed consumption and the production of pork and lard, based on published and unpublished experiments and cost of production surveys by some of the Corn Belt experiment stations and by the United States Department of Agriculture.

They show that as the weight of hogs increases, somewhat larger amounts of feed are consumed per unit of gain, but the increase is less than generally recognized. For the gain in weight between 225 lbs. and 250 lbs., only 8 percent more feed units are required per 100 lbs. gain than

are needed to bring a hog up to 225 lbs. (including the feed and gain of the breeding herd). Even between 275 and 300 lbs., only 18 per cent more feed is required per 100 lbs. of gain than for the 225-lb. hog.

They conclude that the additional amounts of feed required per 100 lbs. of gain up to a weight of 300 lbs. is more than offset by the facts that the percentage of dressed carcass and especially the percentage of edible carcass steadily increase as the pigs fatten. They estimate that under Corn Belt conditions each 100 feed units will yield the following amounts of total lard and standardized pork (not including bone): To 200 lbs. live weight, 13.5 lbs. of edible product; to 225 lbs., 13.7 lbs.; to 250 lbs., 13.9 lbs.; to 275 lbs., 14.0 lbs.; and to 300 lbs., 14.1 lbs. These estimates include the feed consumed by the breeding herd.

Note that when pigs are carried to a weight of 300 lbs. (decidedly over the usual market weights) they will, according to these studies, yield more edible product per feed unit than at lighter weights. The edible portion of the carcass of the 300-lb. pig will also have a higher energy value per pound than in the case of pigs of lighter weights, because it will have a larger percentage of fat and a smaller percentage of water. Therefore, from the standpoint of energy production for human food, the efficiency of our pigs increases up to a weight of 300 lbs. Sufficient data for still heavier weights are not available to warrant conclusions.

The proportion of lean meat in the edible part of the carcass steadily decreases as the pig gets heavier and fatter. For example, the yield of 14.1 lbs. of lard plus standardized pork per 100 feed units at a live weight of 300 lbs. is made up of 5.8 lbs. lard and 8.3 lbs. pork cuts. At the weight of 200 lbs., the total yield of 13.5 lbs. is made up of 3.0 lbs. lard and 10.5 lbs. pork cuts.

Since the efficiency of our modern hogs in total food production, measured on the basis of energy, does not decrease up to a live weight of 300 lbs., the production of pork and of lard can be readily adjusted to meet our needs. When there is a heavy demand for lard for our domestic use or for export, hogs will naturally be carried to heavier weights. On the other hand, when there is a lesser demand for lard, the same total amount of feed can be used for the raising of a greater number of hogs to lighter weights. This will produce approximately the same amount of total edible pork products, but will produce decidedly more lean meat and total pork cuts, but considerably less lard.

Summary:— The proportion of total human food which comes from animal sources ranges all the way from 3 percent on the dry basis in Asia to 25 percent in North America and 36 percent in Oceania.

Can the proportion of food of animal origin possibly be increased throughout the world to the North American level? Since this appears impossible, should we live near the Asiatic level?

Considering all factors, milk production easily ranks first in importance and in efficiency of production among foods of animal origin. Undoubtedly, a further increase in the consumption of milk and other dairy products in this country would be beneficial.

From the energy standpoint, pigs would rank second to milk production in efficiency, but considering all factors second place should probably be given to egg production.

From the standpoint of national food economy, the production of beef and lamb also ranks very high, insofar as the meat is produced largely from forage that would otherwise be wasted.

A liberal amount of foods of animal origin is desirable in our diet for reasons entirely apart from nutritional considerations. Farm animals are an efficient means of condensing and storing a surplus of feed in a period of great plenty. Any pronounced reduction in animal products would greatly reduce the number of family farms. The processing and distribution of animal products provide much more employment and industrial development than would result if we lived almost entirely on cereals and other foods of plant origin.

The supply of milk can be greatly increased in the United States if all farmers follow the methods and practices now used by the farmers in our Dairy Herd Improvement Associations. This increase in milk production can be brought about through better breeding and improved forage production, without decreasing the yield of cereals for human food.

Our beef and lamb production can be materially increased, as need arises and prices for products justify, through the development of good pastures and hay lands on areas not now used for crop production.

Carrying beef cattle beyond the carcass grade of Good is wasteful of feed. On the other hand, our modern type of hogs can be carried to a market weight of 300 lbs. without decreasing the efficiency of food production.

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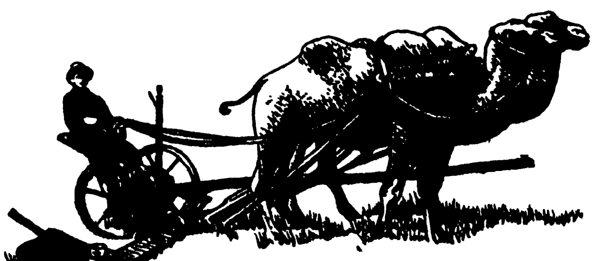

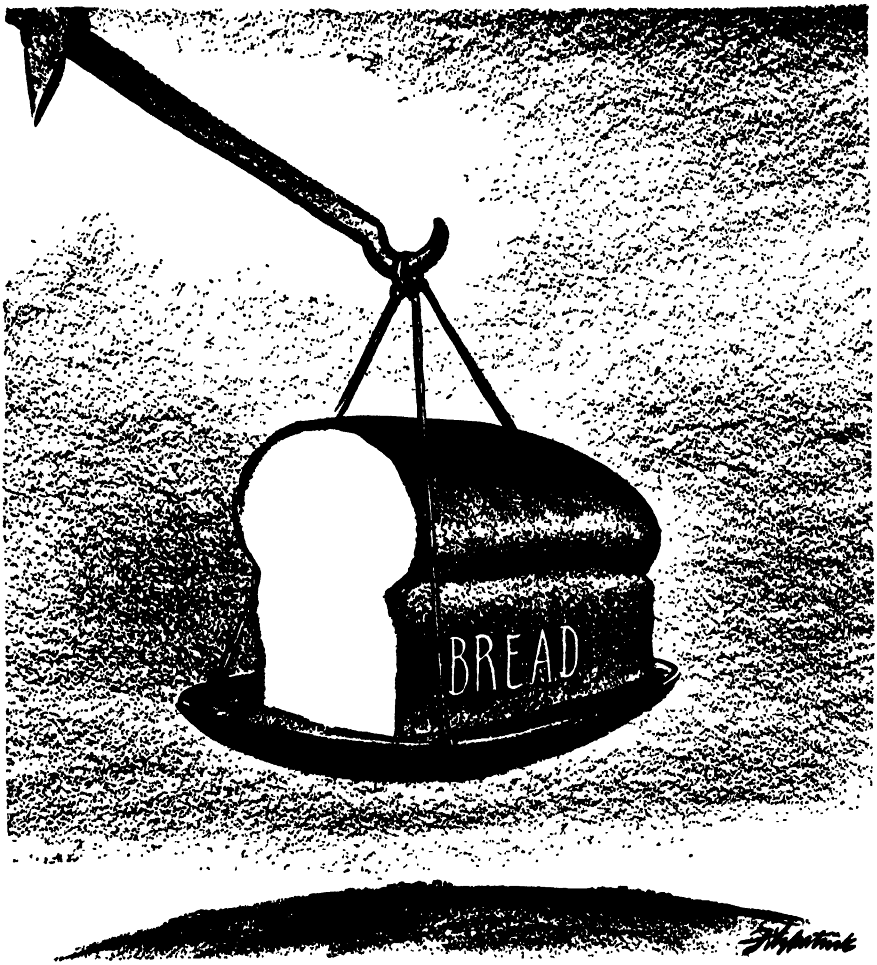

THE NEW BALANCE OF POWER

(courtesy of St. Louis Post Dispatch)

THE ECONOMICS OF FREEDOM FROM WANT

by JOHN D. BLACK

Henry Lee Professor of Economics,
Harvard University

THOSE who planned this program seem to have realized fully that if the resources of the earth are to be utilized so as to achieve a larger measure of freedom from want than now exists, they must be fitted into an economic order as well as into a biological one. The central concept of economics is *use*. The complete and final object of all economic analysis and all application of economic principles is the use of natural resources and human effort in such a way as to obtain from them the highest level of well-being for mankind.

If such analysis is to point the way to such an achievement, however, it must at every step combine natural science with economics and the other social sciences. The papers presented this forenoon, and Dr. MORRISON'S discussion of the animal production this afternoon, have brought before us the basic technological facts and relationships that must be combined with the economic and social in arriving at the final answers sought. They have, in fact, done more than this; they have in addition gone a considerable part of the way in the necessary integrating of the technological and social. And they have done this without falling into the usual errors of scientists crossing the boundaries into adjoining fields.

A word more is important at this point as to the great need of coordinating technological and social science in dealing with actual problems. My fellow economists commonly have so scanty a knowledge of production processes and the like that they are usually unable themselves to apply the principles which they espouse. Equally important, they are in consequence unable to state their propositions in form so that others can apply them. That this need not be the case was proved at a session of the American Farm Economic Association on "Research Needed in Farm and Home Planning" at Philadelphia last Friday. CHARLES E. KELLOGG read a paper at this session on the research needed in the natural sciences, and GEORGE WESTCOTT of Massachusetts State College on the research needed in economics. These two papers were then reviewed by LELAND ALLBAUGH who is now devoting his energies largely to farm planning on TVA unit-test-demonstration farms. His opening remark was that KELLOGG had included about as much social science as technology in his paper, and WESTCOTT about as much technology as economics.[1] The present strong emphasis on farm planning is contributing importantly to such a result. The papers just presented show how the leaven is working among the natural scientists. It is also significant that several of the studies that Dr. MORRISON has drawn upon for his paper were made by economists —

[1] Possibly this effect is to be attributed in some small measure to the fact that both men had occasion to read, in advance of publication, the manuscript of a new textbook on "Farm Management" which makes a special effort to integrate natural and social sciences. (Macmillan, 1947.)

the studies, for example, of JENNINGS and of ATKINSON and KLEIN of the Bureau of Agricultural Economics. But these men in making their studies had the help at every step in their analyses of the natural scientists in the other bureaus of the U. S. Department of Agriculture.

One of the most important forms which coordination of natural and social sciences must take is illustrated by the question that was raised from the floor at the end of the morning session. Of what value, it was asked, is it to develop pest controls that will raise the yields of potatoes perhaps a fifth if as a result we get a large output of potatoes that have no market? The facts are plain enough. This country produced 470 million bushels of potatoes in 1946, and all that it normally consumes is 370 millions. The extra 100 millions were made into alcohol, or fed to livestock, or allowed to freeze or rot. The government paid out around $100 millions in support prices. If the full 470 million bushels had been thrown on the market with no support prices, the price, according to BAE estimates, would have been around 65 cents per bushel. The price received by farmers averaged around $1.35. The additional $.70 per bushel, multiplied into 470 million bushels, made a tidy sum of around $300 million extra income for the potato growers, at a cost to the government of $100 millions, less extra income taxes paid by the potato growers, and less the extra taxes collected on beverage alcohol. The major burden on the public, however, was not in the net cost to the government paid out of tax collections, but the extra $300 millions paid by consumers for their food. Obviously, this country cannot afford to continue such a program indefinitely.

Nor does it intend to do so. The United Kingdom and all the countries of Western Europe grew large crops of potatoes during the war, and are still doing so. If they had not, they would have faced real starvation in some years even before the war was over, and surely since. The potato crop is the best possible form of insurance against failure of the cereal crops. This country, in planning its 1947 production program last winter, could not safely count on good yields of wheat, corn, rice, and other cereals in this and other countries. If a potato surplus arises when the yields proved to be good, this is a small premium indeed on the insurance policy.[2]

The large 1946 potato crop in this country was due only in part to improved techniques of pest control. Favorable weather and increased acreage were other factors. Lowering the support price as soon as the world's food supply seems reasonably assured is the obvious way of reducing acreage. It will, in this case, need to be reduced enough to offset the higher yields in prospect. If the Steagall Act of 1942, or perhaps some new legislation, prevents lowering prices as much as need be, a limit will have to be placed on the size of crop receiving the guaranteed price.

If this country continues its various forms of production control, or price supports, or both, in the years ahead, it will need to work out a similar balancing of acreages, yields, prices and incomes for all its crops. If advances in technology raise yields, lowering costs at the same time, more will be consumed in this country, and probably more exported of some farm products. Prices can be lowered and still net farm incomes will rise. Yet

[2] The consequences of a short crop of potatoes in Europe are forcibly illustrated by the 1947-48 experience. Unfavorable weather may come any year, and the only protection against it, in a critical period like the present, is to plant larger acreages of staple crops than normally needed.

it will be necessary to restrict acreages unless the prices are set lower than the producers of farm products are now disposed to accept, or measures are adopted for distributing large additional amounts outside the usual channels of domestic and foreign trade.

The problem will be not greatly different if this country abandons its attempts at specific and direct production and price control and allows prices to be set in the pre-1929 types of markets for farm products. We shall then only substitute other types of control for the present specific and direct ones — controls taking the form mainly of providing more public information and guidance to farmers in adjusting their production to the market, of aids and services of various kinds in making these adjustments, including particularly credit aids, and of the necessary supporting research. All of these aids are most effectively combined in the form of assistance in farm planning, of helping farmers to analyze different alternative organizations of their farm businesses to see which, taking account of impending changes in relative demands and prices for different products, and changing wages, cost-rates and technologies, promises to return the largest net farm family income.

Aid to farmers in production adjustments was definitely pointed in the direction just described in the 1921-29 period. It was in these years that the "outlook" program was developed. Perhaps the introduction of the Christgau Bill in 1928 was the climax point in this movement. This bill would have set up and financed a program for developing farm budgeting analysis by type-of-farming areas, under federal-state collaboration, for the whole country. With the crash of 1929-32 came direct production control measures instead.

Events since 1928-29, however, have made it apparent that guidance to farmers in production adjustment, if it is to be effective, must be more specific and more positive than was understood at that time. Something in the nature of a production goals program surely needs to be continued — the issue really is only a question of how to implement such a goals program.

Integrated with such production adjustment must be some important developments in marketing and consumption adjustment. Since 1910, the number of workers engaged in distribution in this country has increased 80 percent, and the number in all forms of production less than 20 percent. In spite of all the developments in transportation and storage, in cooperative marketing, in chain store distribution and the like, the distributors' share of the consumer's food dollar was around 60 percent in 1938-40 compared with 50 percent in 1910-14. The program of marketing research and services now authorized in the Hope-Flannagan Act must be pointed not only at reducing the amount of human and other resources that go into getting farm products from the primary producer to the final consumer, but also at relating production and distribution more closely to each other.

Developments in consumption adjustment must similarly provide for a better fitting of production and consumption to each other. But more important is it that they provide methods by which better nutrition is made possible for the malnourished and vulnerable groups in our own and other populations.

This is not the place for detailed presentation of the dependence of the foregoing upon the effective working of the general economy. For this, the reader is referred to Professor T. W. SCHULTZ's recent book, *"Agri-*

culture in an Unstable Economy." Professor SCHULTZ deals primarily with two aspects of this, one secular and the other cyclical. Under the first, he points out that with no better organization of distribution and consumption than in the past, industry in the United States must expand from two to three times the rate for agriculture if surpluses of farm products are not to arise. Under the second, he points to the need either for preventing the recurrence of periods of mass unemployment, or for a form of combined subsidizing of agricultural production and food consumption in the years of depression.

The international aspects of these problems are now being considered by the Preparatory Commission set up to consider the World Food Board proposal put forward by FAO. What this Commission proposes will be only a beginning. Even if, and after, its recommendations are accepted at the next annual FAO conference, they will still be only words. The actual progress achieved at that moment will be measured by the extent that the thinking of the participating countries has advanced toward an understanding of the means of successful implementation of FAO's broad objectives. That progress will be indicated by the measures actually taken in the next ensuing years. These may not have too close a relation to the words of the recommendations. To illustrate, a vigorous FAO leadership in promoting school feeding programs might lead to a rapid development of these in most of Europe, in Japan, China, and India, and in much of South and Central America and the East Indies, with production programs adjusted to provide the protective foods needed for school feeding; and such a development might mean much more for agriculture and the human race than any proposal for direct stabilization of farm prices through international trade and commodity agreements.[8]

The point has been reached in this analysis where it is necessary to relate it more directly to the conception of freedom from want. As we go about this, we need always to bear in mind that freedom from want is only *relative,* and relative is a very broad term. Freedom from want is, in fact, only a slogan or catch phrase. In the best possible world, there would still be about as many things sought after as now. A world in which the human race had lost all its aspirations, and was no longer striving for more and better things, would be a sorry world indeed. Some persons have used statements such as the foregoing to ridicule the whole conception of freedom from want. I do not introduce it for this purpose, but instead merely to keep our thinking straight. In the better world for which we are seeking, however, our wants will be of a different composition than in a world in which more than half the population is malnourished. There will still be less even of foods of many kinds than people would buy if they had the means to buy or produce them. We would not even like a world in which food was a free good, as air and sunshine are often, but mistakenly, said to be. We would, however, like to be able to produce enough food so that no one need have poor health or lack strength and endurance because of not enough foods of the right kinds. There still would be more varied and more luxurious diets for those willing and able to pay for them.

We need always also to keep in mind that wants for food compete with all the other wants, not only for consumers' dollars, but for the use of our

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[8] A World Food Council was provided for at the Geneva FAO meeting in September 1947. It remains to be seen how effective it will be.

natural resources and our human resources. The soil produces clothing and shelter as well as food; and cotton and wood for automobiles, for household furnishings, for fuel, plastics, industrial alcohol, and a growing list of industrial products. As man's productivity rises, the consumption of these will increase along with consumption of food, and for most of them at a faster rate. If we ever get a world in which the population and productive resources are generally in good balance, our land will be producing a larger percentage of clothing and shelter and warmth than now, and a smaller percentage of food.

The same is of course true for the use of all other resources, including human resources. With higher productivity per hour of human labor, relatively less and less of this labor will be used in producing food, and more and more in producing not only clothing, shelter and warmth, but health, education, entertainment, and finally leisure. In this country we have already reached the point where, since 1910, industrial workers have chosen one-fourth fewer working hours and more time for leisure and recreation, in preference to a higher volume of output. We cannot say that these workers have freedom from want, but they do want leisure and free time more than they want what they could buy by working thirteen hours more per week.

The most direct tie-in of the subject of this paper with the papers coming before is with the first one, the one dealing with the relation of population to world food resources. Any fundamental analysis of the economics of freedom from want must run largely in terms of the ratios of populations to resources. Two general sets of principles have been developed on this subject. The first set of principles, as first propounded by MALTHUS, assumed populations living at the point of subsistence. We still have much writing largely in terms of MALTHUS' first "Essay on Population" — for example, the recent book on "World Hunger" by PEARSON and HARPER. (Of course MALTHUS himself later expanded his theory to provide for populations living above the subsistence levels.) The second set of principles centers around the concept of the optimum population, a population with every family living at the highest level of well-being possible at the time in the country. It should be obvious that such a level would be the practical equivalent of the freedom from want that we are here discussing. It would represent the closest possible approach to freedom from want under any given state of the arts. Only by improving the arts would any higher order of freedom from want be attained.

Let us now develop our subject by setting in contrast two populations, one living on the mere subsistence basis, and the other living somewhere near the optimum level of well-being. I shall refer to the first as Malthusian, although this is an injustice to MALTHUS. The Malthusian regions of the earth, so conceived, are those in which the population multiplies all of the time exactly up to the limit. Maybe this limit is correctly referred to as the biological limit, but there may be some confusion about this in a society in which the arts have advanced beyond the primitive stage. If it is true that the amount of food produced, and the numbers of people kept alive, depends upon how the land use is developed, how the production is organized, and what tools, equipment and fertilizers and the like are used, then surely economic and social factors as well as biological enter into determining the population limit. One student of population in the Far East

has observed that given three tracts of land of equal inherent productivity, one in Japan, one in China, and one in India, and each farmed at the state of the agricultural arts that is *average* for these countries, the Japanese tract will produce roughly twice as much as the Chinese tract, and the Chinese tract roughly twice as much as the Indian tract. These differences are far from being merely biological.

But given these three tracts, the population in them will be roughly proportional to the quality of their agriculture — that is, if they are all Malthusian areas. In all three of them, the number of children born and the life expectancy are determined largely by the amount produced of food and other absolute essentials of existence. The population of India, if the census figures are taken at their full face value, increased 52 millions, or 15 percent, between 1931 and 1941. In 1921-31, it increased only 10 percent; and in 1911-21, only 1 percent. The differences in the rates of increase are largely explained by the rate of increase in the food supply,[4] much as the normal population of pine borer beetles in our northern forests is determined by the number of fallen pine trees. When the 1938 hurricane blew down large numbers of trees, there were enough beetles born so that no fallen tree was uninhabited. Thus the more fallen trees, almost without limit, the more beetles. No human populations are able to expand at the beetle rate; but they come near enough to it in the real Malthusian regions so that seldom does the food supply run ahead of the population for more than a few years.

Perhaps as good an indicator of any possible gains made in the face of population pressure is the expectancy of life at birth. Between 1881 and 1931, this figure changed for all females in India only from 25.6 to 26.6 years, and for all males, from 23.7 to 26.9 years. These figures compare with around 65 in a few of the healthiest countries.

In such a society, everybody works who is in the least able, but most of them have strength, because of their low food intake, for only a few hours of real work per day. A balance is reached between the amount of work and the food resulting at the point which supports the *maximum number* of persons, taking into account the additional food needed to bring children into the world and rear them to the point where they produce more than they eat.

The principal factor qualifying the foregoing analysis is the unequal distribution of property and income. The analysis is exactly true only if these are distributed absolutely equally. If there is a landlord class, or any group in society which lives without working, this group becomes an additional burden on those who work, and the population numbers are reduced to that extent.

Population is reduced in such a situation by another important factor. Not all workers in such a society produce the same amount. Some are always more capable or more efficient than others. These are able to handle more and better land and other resources than their neighbors, feed and clothe themselves better and rear more and better children. Only the families with the lowest possible, or marginal, efficiency live at the bare minimum described above. The more efficient families deprive the marginal

[4] The 1918 influenza epidemic killed off perhaps 15 million, because, in large measure, they were malnourished.

families of the better land and other resources, and by so doing probably reduce the total population. \

The distinction between *capacity* and *efficiency* is important here. Getting more resources into the hands of those with high capacity but low efficiency does not increase the average or total product, but rather lowers it, and at the same time reduces the resources available for the rest, and hence reduces the population. But getting the better resources into the hands of the more efficient definitely raises the total and the average product, and takes less away from the inefficient, and hence reduces the population less. Generally speaking, persons with large capacity and only fair efficiency are able to get more than their due share of the resources, by inheritance if not in open competition.

For two important reasons, therefore, under actual conditions even in the Malthusian areas, the population is smaller than the theoretical minimum, and the levels of living *average* a little above the bare minimum.

Rarely do we recognize how inefficient and wasteful of resources and of human effort is a Malthusian society. The ratio of the maintenance to production rations is unbelievably high in such a society. If we assume that the growing child in India at 12 years reaches the point where it produces more than it eats, and allow as need be for those who die between birth and 12 years, only a short span of productive years is left with an average expectancy at birth of 27 years — probably not much more than 20 years. To the maintenance ration must be added, of course, the food consumed in prenatal growth and in growth until death of those dying under 12 years of age. And a large fraction of those of working ages have little left in their diets over and above maintenance rations to convert to useful work.

Now let us consider population at the other extreme, with every family living at the optimum level of well-being.[5] (There is no such people, but we can assume one.) At this point an increase of one percent in the population will reduce the amount per capita of food, clothing and all other things necessary for well-being, because there is slightly less of land, capital and other resources than will give them maximum per-capita output. One percent *fewer* people will also reduce per-capita output, because more workers are needed to use resources with full effectiveness. Such a condition may exist in a new country on the frontier, or among a people who are short of capital, or in certain stages in the history of a people when its arts of production are advancing very rapidly.

Such an optimum society will also have its idle leisured class, producing nothing and living off the product of the rest. *The existence of these, however, does not change the location of the optimum population point, but rather only requires that the workers must share their optimum with the non-workers.*

Differences in productivity between families enter also into such a situation. They work out very simply as follows — each family has its own optimum point, the point at which it gets its largest return for the effort which it is willing to put forth, this point varying with its efficiency and capacity. In a really optimum society, there must be enough land and other resources so that *each family* has enough so that having a little more would reduce its total output. In a situation such as prevails in the United

[5] The optimum is sometimes defined in other terms, such as real income or purchasing power.

States and similar countries, a small fraction of the families would fare better looking after fewer resources. Some farmers, for example, try to operate more land than is best for them. But the great bulk of the people are living under sub-optimum conditions, sub-optimum by a wide margin, even after allowing for the circumstance that those with low capacity and efficiency have low optima.

It follows from the foregoing that the optimum population for a country has to be an average of the optima for a wide range of different productivities, ranging in fact from almost zero to very high indeed. In fact, the only way that a nation can be sure that all its families get enough to eat and wear to keep them well is for those with high optima and high outputs to share some of their incomes with the ineffective and indigent. Hence our systems of progressive income taxes based upon ability to pay.

Where do the countries of the earth lie with respect to the two extremes defined, of maximum population under Malthusian conditions, and of optimum population? More than half the people of the earth live under the first or very close to it, if we allow for the two qualifications of effects of the unequal distribution of control of resources and the differential abilities of families. The tenure systems of Malthusian countries may allow a small fraction of the population to receive a sizeable share of the national income. Of course they have no way of spending much of this except by hiring servants in their own countries, or perhaps by building castles or temples with it, and they have to feed the workers if they are to serve them or build for them. Still a good deal of energy goes into these things that might produce food instead and hence support more people.

It may be argued, of course, that a better distribution of wealth, making it possible for more people to live under Malthusian conditions, would be no gain to any nation or to humanity in general. Why multiply misery? But one needs to consider also what use the rich make of their higher incomes. There surely are forms of culture that mean more for humanity and civilization than more people living in misery. But much of the present use of riches is degrading rather than upbuilding.

In the countries with the highest ratio of resources to population, even after allowing for the low optima of the families with low efficiency, a large fraction of the families are operating below their optima. The national average of well-being would be raised if they had more land and other resources to use. This description certainly fits half the farm families in the United States, and three-fourths of those in the South. In countries like those of Western Europe — France, Belgium, etc. — the fraction of families living below their optima is larger than in the United States, but not than in our South. In Spain, Italy and Greece, however, the fraction is larger than in our South.

Perhaps at this point it is necessary again to state that freedom from want is only a relative term. Even the optima which are reached with an ideal balancing of population and resources will not give the people of a country complete freedom from want. There are still more things they would have if their optima were higher — if nothing else, then more leisure and free time.

The foregoing discussion has assumed the existing state of the arts and technologies in each of the countries. These can be advanced in all of them, and with this advance the optima rise. The people of this country living

at their optima today have many more comforts and pleasures in their lives than those living under their optima a century ago. And the agricultural arts are on a much higher level in some parts of this country than in others.

It is entirely reasonable, it follows from the foregoing, to talk about increasing the amount of freedom from want. Consider the following list of ways and means:

1) Adjusting the numbers of population so that each family can come closer to its optimum output and accompanying level of living.
2) Improving the arts and technologies, not only in agricultural production, but in the distribution and use of farm products; not only in agriculture, but in all lines of production and use. Industry has made much larger contributions in the last century to raising the levels of living in agriculture than agriculture has to raising levels of living of industrial workers.
3) Increasing the productivity of the workers, by enabling them to feed themselves better and keep themselves in better health, and by educating and training them to increase their skills and efficiency.
4) Developing the productive resources of a country, by land improvements of all kinds, by utilization of waterpowers, by better conservation of resources.
5) Included in the foregoing, but probably needing special mention is increasing the *amount* of capital goods used per worker, in the form of power in all its forms, of tools, machinery and equipment, of buildings, of fertilizers, of livestock and feeds, etc.

That the possibilities in lines 2, 3, 4 and 5 are very great, has been made very clear by the papers presented today. They could more than double the average world output per worker in the next century. But only if the number of workers and bodies to be fed and clothed do not increase except where and when they need to do so in order to use resources with full effectiveness. *If the optima are to be raised, the gains along lines 2 to 5 must be faster than the population increase.* The rise of the optima will be a function of the difference between the two rates of increase.

It is at this point that we need to tie our analysis with that of Dr. SALTER's bold and excellent estimate of the world's food-producing potential and the balancing of his estimates with the 1960 food needs of the FAO World Food Survey. We need to keep in mind in this connection that the 1960 food needs assumed a world population increase of 25 percent in the 25 years from 1935 to 1960 — that is, at the compounded rate of 0.91 percent annually. Dr. SALTER's increase in prospect from better agriculture on present acres, it will be remembered, was not enough to take care of this 25 percent population increase, even with a considerable deterioration of the diets by shifting from protective foods to roots and tubers and cereals. Only by drawing considerably upon a reserve of 1300 million acres of new land, mostly in the tropics, could the 1960 food targets be attained. And these targets, it will be recalled, allow only 2600 calories per day for half of the world's population, compared with 3240 calories per day consumed by the North American countries before the war.

Now let us assume that the world's population increases another 25 percent by 1985 — what then about the balance? Long before then, the potential increase in output of pulses, milk, meat and fruits and vegetables will have been exhausted, and the population will be living on a diet much below the targets in everything except cereals, roots and tubers, sugar and fats and oils. And how about still another 25 years?

It is clear from these simple facts and figures that far more vital to the success of FAO's program than technological progress in agriculture, or

bringing new lands into farms, is what happens to birthrates and deathrates in the next half century. All hope of any progress toward freedom from want for food in the world could be buried in a population avalanche.

It was by such a putting together of data that EDWARD M. EAST after the first world war came to make the alarming forecasts of his "Mankind at the Crossroads," and that his former student, Professor KARL SAX of Harvard, arrived at his conclusions in the article in *Science* that was reprinted in the *Congressional Record,* as an attack on the pronouncements of the Hot Springs Food and Agriculture Conference; that E. PARMALEE PRENTICE in "Food, War, and the Future" concluded that population pressure caused both world wars and will cause another war unless agricultural technology, especially genetics technology, improves rapidly; and that PEARSON and HARPER recently got themselves in a similar state of mind in their book "World Hunger."

Dr. SALTER, it must be emphasized, does not in his paper state any conclusions of this nature. He purposely refrained from looking beyond 1960. He realized that the problem is far from being as simple as the foregoing would seem to suggest. The all-compelling other circumstance is that an analysis of population problems in terms of world totals has only limited significance in the world at present. Considerable food does move from one country to another, it is true; but the adjustment of population to resources is still largely a matter for each country by itself. Mr. TOLLEY pointed out in his paper that the nations in which forty percent of the world's population live have already achieved a fairly good ratio of population to their resources. A more precise statement would be that before the Second World War, a majority of the people of these countries were moving *toward* an optimum population level rather than away from it; or that in them generally the arts were advancing more rapidly than the population. In fact, this had been true of all the industrial nations of the world ever since the Industrial Revolution got fully under way in them. Mr. TOLLEY also explained how in the first part of the industrialization of a country the population increases rapidly. This is partly because better incomes provide better diets and health and reduce deathrates, but also because marriage rates and birthrates increase also for a while. Presently, however, a point is reached when the birthrates decline along with the deathrates and the population growth begins to level out, as it has in Japan in recent decades. Europe as a whole, excluding Russia, has advanced far enough along this route, so that little population increase is expected after 1960, according to the Office of Population Research of Princeton University.[6] Russia was in the earlier stages of such a development when the new world war hit her. She is expected to have a large population growth for several decades, but to reach a peak of around 300 million in the year 2000.

The true picture of the population balance in the world is therefore not one of all the world sinking toward a food-subsistence level, but instead of one country after another working itself into a state in which the arts advance faster than the population, output per worker increases, and the country definitely begins to move toward its population optimum. The forty percent which Mr. TOLLEY mentioned will become fifty percent in a few decades, then 60 percent, etc.

[6] NOTESTEIN, *et al.*, "The Future Population of Europe and the Soviet Union."

How long before countries like China and India will start on their way toward their optima? They, as a matter of fact, probably have but one direction in which to go. They probably are already at their lowest possible limit, as that limit was defined earlier. But they are likely to rise from this level rather slowly at the start — even if they make mighty strides in agricultural technology and industrialization. The gains for a few decades will mainly take the form of less disease and misery and longer, healthier lives. The accompanying rise in the ratio of production to maintenance ration will presently, however, begin to manifest itself in rising levels of living, and this in a few decades by declining birthrates. Unless this stage is presently reached, the gains will be lost and the countries will start backwards and downwards again away from their optima.

It follows from the foregoing that each country must work out a solution for itself along the lines indicated. No country can do it for another. Such a statement is wholly in keeping with the pronouncements of the Hot Springs Conference on Food and Agriculture. Number one of these pronouncements was that enough food can be produced in the world to give all peoples the food they need for health and working efficiency. Number two was that the task of doing this is the responsibility of each nation. Number three was that the task can be done more effectively, however, if the nations help each other to do it.

The Hot Springs report says little, however, about the population aspects of the problem. It mentions industrialization and migration as ways of adjusting population resources, but says nothing about reducing the birthrate, which is the crux of the problem. The reason that birthrates were not discussed at Hot Springs is that they were not deemed a safe subject to talk about in an international gathering. They are tangled up with religion in some countries.

A few of the overpopulated countries envision emigration as a means of solving their problems, pointing to the way in which emigration from Europe to the Americas relieved population pressure in Europe. As long as any new country is so thinly populated as not to be able to utilize its resources to the optimum point, it may welcome immigration. But most of the newer countries already have all the people they need to develop any remaining areas. Some of the Latin American countries, it is true, may point to their vast undeveloped resources and say that they need more people to develop these. Actually much of their present population may be living at relatively low levels — almost like those of Old Spain. What they really lack is capital and technology and skills rather than people. Internal migration out of their own already congested areas would provide all the people they need without any increase in birthrate or immigration.

Some of the newer countries have visions of themselves as great industrial nations like the United Kingdom or the Germany that was. Few of them have the resources for it. If economic principles have any validity at all, they posit for each country, as a necessary condition for a high degree of freedom from want, a reliance upon those lines of production in which nature has endowed them most abundantly, and if this gives them more cotton, or wheat, or beef, or olive oil, or raisins, or sugar, or rubber, or tin, or pulpwood, than they can use at home to advantage, to export these in exchange for goods for which they do not have adequate resources. It is true that some of these countries can have larger populations by

subsidizing industries that use much cheap labor; but only by lowering the scale of living of their people, by accepting less rather than more freedom from want. What do they gain by such a course of action?

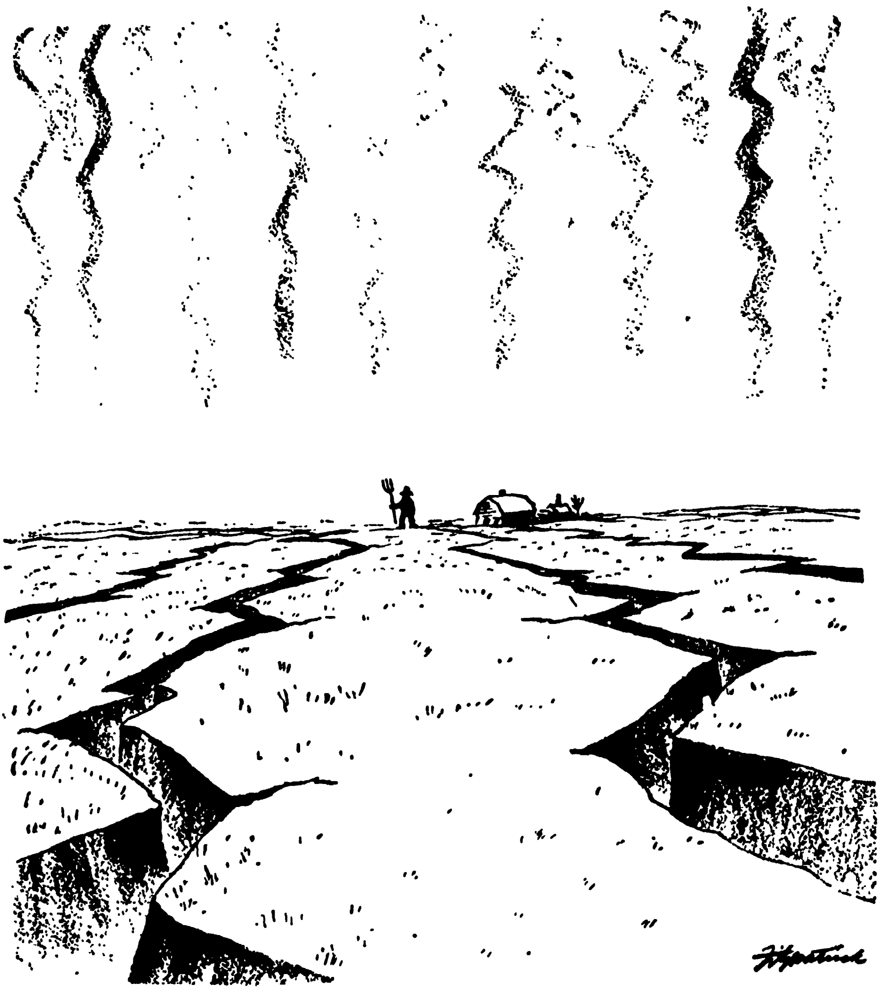
Confronting any nation at any stage are alternatives as to what measures to promote because contributing most to freedom from want. This is a major task for the economic statesmen of each country. The choice can very easily be wrong. Judged from the standpoint of freedom from want as a criterion, the best choice is always the one that raises the average output per worker the most in the direction of the optimum. No land developments should be undertaken which are so costly that they lower the optimum.

One may ask, for example, whether the Dutch were doing this when they built the big dike across the Zuider Zee in order to make possibly 400,000 more acres of land available, at a cost of perhaps $600 per acre, when the agriculture to be practiced on this is to consist, at least for a long time, of dairying plus sugar beets plus wheat, the first for export and the other two under heavy subsidy. Or whether the best use of agricultural resources in the United States is to subsidize cotton production in order to maintain the present rural population.

International measures proposed must be analyzed from the point of view of the foregoing considerations. Will they help the countries affected or concerned to move toward their optima or away from it? Let us consider, for example, an international school-feeding program from this point of view. Feeding children of school age certainly will not increase the birthrate. On the contrary, probably the more children that can be brought into schools, the more rapid the spread of understanding and desire to raise the levels of living. Deathrates will surely decline as the health of the young people is improved, and this will increase the population. But the gains from the improved ratios of production to maintenance rations will more than offset the effects of the population increase — that is, wherever gains are possible from the application of more labor to existing resources. Feeding pre-school children will work out in much the same way. Such supplementary feeding programs are *selective*. It may be doubtful, however, whether promiscuous supplementary feeding of the malnourished will be more than a palliative in many congested areas.

From such an interpretation of economic and population history as last presented, one is able to arise with vision of hope for mankind. One can also see in it an opportunity for improved technology to contribute in a large way to an increase in freedom from want. In fact, in such a world the more rapid the improvement of the agricultural arts, the larger the balance in favor of rising levels of living.

Corollary to the foregoing, one is able to envision, in all those countries which are moving toward their optima, a rising level of food consumption; and this rising level of food consumption will include a larger relative intake of milk, meat, fruits and vegetables and other protective foods — such a rising level as Europe generally experienced in the 19th century, and at least until the First World War. The gains in this direction will for a long time be very small, however, in the oriental countries now obtaining 90 per cent or more of their calories from cereals, roots and tubers.



Farm Problem Number 999,000

(courtesy of St. Louis Post Dispatch)

OBLIGATIONS OF SCIENCE TOWARD FREEDOM FROM WANT

by M. A. McCall

Asst. Chief, Bureau of Plant Industry, Soils and Agricultural Engineering, U. S. Dept. of Agriculture

Science alone cannot bring about "Freedom from Want." On the other hand, science can and must play a prominent rôle in doing the job. Certainly agricultural science has the definite responsibility to develop methods and materials for producing food of the qualities and in the quantities necessary properly to nourish the world's people. Science also has the responsibility to stabilize food production by removing hazards so that we will not be faced at any time with the specter of "too little." Likewise on science rests the obligation to increase efficiency of production to the point that ample food can be produced profitably at a price low enough to insure that it will be available in adequate amounts to everyone on whatever economic level. Science too must provide the means to produce our food supplies on a permanently sustained basis. And finally science must play its part in helping to adjust its contributions within existing political, social, and economic structures so as to insure effective use.

Recent records in the United States give reason for optimism as to the contributions which science through technology has made and can continue to make to national and world food supplies. The Bureau of Agricultural Economics reports that during the 5 crop years beginning with 1942 our farmers produced enough food each year to nourish approximately 50,000,000 more people than could have been fed at comparable dietary levels from our national production during the last 5 years of the 1930's. This job was done with 10 percent fewer workers. Some of this larger production was due to favorable weather, but by far the larger part can be credited to technological advances due to science. In 1942 weather was unusually good and the crop of that year set a new record. In 1943, on the other hand, low temperatures and excessive rainfall extending well into early summer delayed planting and early crop development, yet in spite of unfavorable weather the crop was larger than in 1942. The 1946 crop with only average conditions over the country as a whole was the largest ever grown in the United States.

The technological advances responsible for improved crop yields, and consequently for a better national food supply, fall into three categories: (*1*) increased mechanization, which reduces manpower and cost requirements, releases substantial quantities of feed for producing meat, milk and poultry food products, and which by speeding up operations and permitting each successive step to be taken at the optimum time increases yield; (*2*) an increased use of lime and fertilizer in conjunction with general average improvement in soil management, still far below desirable levels; and (*3*) the widespread use of high yielding hybrid corn and superior disease resistant varieties of wheat, oats, soybeans, flaxseed, potatoes, and other crops.

This remarkable record is one for which every citizen of this land should be deeply grateful. During the war it gave ample food for our civil population and armed forces and substantial amounts for our allies. In total it gave the edge in power necessary to achieve victory. Now as a result of our own abundance we can help others in desperate need.

In spite of concrete elements of progress, the record to date should not encourage complacency on the part of anyone, particularly the agricultural scientist. There are too many components in the overall situation that are latent threats for the future, too much responsibility to maintain advances, for anyone to rest on his oars.

At first glance increased yields and larger immediate food supplies during the present period of world necessity overshadow other connotations of equal or even greater import in a permanent "Freedom from Want." In noting the factors responsible for our greatly expanded production, the increased use of lime and fertilizers and some improvement in soil management were included. As a matter of actual fact this contribution to increased total production and permanency have both been decidedly less than should be true. In comparison the total stemming from the immense increase potentials of hybrid corn and the new disease resistant crop varieties with the smaller amount from mechanization and its better timing is much more significant. In the long pull this is serious. We are in reality accelerating depletion of the soil, our primary food producing resource. It is obvious that larger yields remove larger amounts of essential soil nutrients. This in turn hastens the day when soil resources may become a limiting factor in determining total food supply to a much greater degree than at present. Already the heavy drain of increased yields from hybrid corn and other improved crops is beginning to show its effects in areas of our own highly fertile Corn Belt where heretofore declining productivity has not been a dominant factor. In 1946 the hybrid corn crop in some sections of the Corn Belt showed decided symptoms of nitrogen deficiency, even with rather substantial additions of nitrogen fertilizer. Ten years ago these same soils showed only minor benefits from nitrogen applications. It is true that growing hybrid corn does not necessarily mean removing more nitrogen from the soil than growing an open pollinated crop. While the lower yields of open pollinated corn require less nitrogen than the hybrid crop, the balance of the nitrogen made available by cultivation is leached away. Hybrid corn on the other hand probably uses more of the available nitrogen with less loss by leaching, but with an ability to use even more. This heavy drain on soil nutrients, as evidenced by nitrogen, is occurring also with phosphorus and potassium. While not equally evident in all areas, this sequence of events with respect to soil nutrients is operating where ever the larger yielding crops are grown. The problem while not equally acute in all sections, is universally present.

Some may argue that displacements incident to emphasis on maximum production of food and feed crops needed to meet the war emergency are responsible for present developments, and that normal readjustment will meet the situation. Bureau of Agricultural Economics crop estimate data do not show a change in over-all cropping pattern that supports such a view. There were changes in certain areas as a part of the war program that have been inimical to long time sustained production. For the country as a whole, however, this does not seem to be true. During and since the

war the acreage of sod crops, including the perennial legumes, alfalfa and the clovers, has not been materially different than that immediately preceding this period. All will agree that these crops, so necessary in a sound permanent and sustained agriculture have always occupied too small a place in our land use pattern, but for the country as a whole, the situation is not worse because of the war. The same failure to use the best knowledge and practices exists in relation to other factors of soil management necessary to build up and maintain our soils under use. We admit too little information regarding this most important problem, but use on farms lags far behind what we do know.

The lag in use on farms of the best practices of soil management is primarily due to failure of scientists to transfer the basic principles of soil and crop science into systems of management workable on farms under a wide variety of conditions. Inadequate methods for educating and training farmers in the use of the better systems of management also contributes. We dodge the issue if we ascribe slow acceptance to inertia on the part of farmers in accepting and using sound land use, management, and cropping information. We must recognize that the result stems back to certain omissions on the part of ourselves as scientists. The correction of these omissions is an important immediate responsibility of science in attacking the long time problems of "Freedom from Want."

The very prompt acceptance of hybrid corn and the new disease resistant field crops on the part of farmers demonstrates that they are not unwilling to accept the new and better. This acceptance was practically universal as shown by the almost complete disappearance of open pollinated corn from the Corn Belt in less than 10 years. The immediate and widespread increase in Tama, Vicland, and Clinton oats, and of Thatcher, Rival, Pilot, and other wheats likewise forced older varieties at once out of the picture. The difference between acceptance of these advances and of similar advances in the field of soil management promising equally great increases in yield and more permanency poses an important problem.

The reasons for difference in acceptance are relatively simple. The answer to the problem they raise is less so. Growing a new and better variety of a crop already being produced requires no change in farm organization, management, utilization, or general plan of financing. When the farmer is convinced the new variety is better, it is easy to make the change with no dislocations. Original growers of the new variety, through their county agent or the experiment station, usually have a chance to see it growing on the station plots where its values are readily demonstrated. Neighbors of original growers see the new variety under circumstances where its advantages are immediately evident. The sequence of demonstration, acceptance, and use is natural, easy, and almost inevitable. Any demonstrated change for the better that does not involve radical change in organization is very readily accepted by the farmer, be it a new crop like soybeans, a new variety like Clinton oats, or even something so completely new in concept as hybrid corn.

The soil management picture is more complex and the basis for farmer acceptance not so simple. During the period that saw the introduction and spread of hybrid corn the Federal government spent substantial sums in a program designed to build up and conserve the soils of the United States. This program included encouragement in growing grasses and

legumes, and the use of lime and phosphate. Farmers are now using three times the lime and twice the fertilizer they used 10 years ago. This is a substantial increase, but in terms of all soils needing such treatments it is still relatively small. The grass and perennial legume acreage, essential in any sound soil maintenance program, presents a picture which is even less encouraging than that for lime and fertilizer. In 1929 the combined alfalfa-clover-timothy acreage was 41,396,000 acres. In 1941 these crops were grown on 34,287,000 acres. In 1946 they occupied 38,716,000 acres. That the reduction from the 1929 totals was not due to the war is shown by the 1941 acreage, which was less than that of 1946.

What is the reason for this apparent inability to get more in tangible results from the soil building program? In the writer's opinion, it is due to a complex series of factors, some of them primarily sins of omission on the part of agricultural science and its administrators. Being an administrator in this field, the writer admits his share of guilt.

When a new product or process is developed in commercial industry, it is axiomatic that an oftentimes extended and expensive period of pilot plant study is necessary before the final economically workable process or equipment is developed from original principles and procedures. This is true no matter how sound and workable the new development may be.

In the field of agriculture, the research scientist has thrown the entire responsibility and expense for the pilot plant stage upon the individual farmer. The farmer is given only the original patent, as it were, and he must adjust the new principles to his own farm and operations. It is true that he may see a preliminary working model which demonstrates the principles on experiment station plots, but this may have little relation to his present farm plan and economic set up. It is up to him to work out the new operation schedules and how he will convert to them. This may require a more or less complete revision of his farm set up. It may mean a change in constituent enterprises in the farm plan and the necessity for him to learn all the basic requirements of the new enterprises with an attendant probability of loss during the learning period. It may require special financing to provide equipment to handle new crops or practices, livestock for utilizing new crops, new buildings and fencing to care for livestock and their feed, developing new market contacts, and a host of related problems. A farmer faced with such a situation is naturally conservative about change except as it fits rather easily into his present set up. In most cases he cannot afford to be otherwise.

Some farmers, more able, more venturesome, or more fortunate, do work out the pilot stages and adopt the new practices in different variations into sound workable systems. The farms of such men serve as demonstrations, and through natural observation on the part of other farmers and the work of county agents, the practice spreads. But the rate of spread is too slow, and often at too great cost to individual farmers. In terms of national need, this system is ineffective. It also results in failure to reach, in any effective way, some two-thirds or more of our farmers at all.

In sections of the country where mixed types of farming predominate, the problem of bridging the gap between soil management research and farm use is less acute. In such cases less adjustment is usually required in cropping plan or in farm enterprise. On the other hand, in single crop

areas like the Cotton South where improvement in soil management is acutely needed, the problem is most serious.

Most of us are strongly skeptical of anything savoring of the old demonstration farm idea. In too many cases we have seen these farms, set up to show how to operate a farm efficiently, end in failure. Certain modifications of this general idea, however, seem to offer possibilities for better implementing research. The Alabama Agricultural Experiment Station has set up a farm where the findings of the Station as to the best rotation, cropping and soil management practices are worked into a farm plan, including cotton and other enterprises. The entire plan is developed to parallel as largely as possible what any farmer on a similar farm could do in converting from a single cotton crop enterprise to a more diversified farm plan, including sound soil management. It provides for conversion by degrees so that learning new enterprises and financing can be cared for safely. It offers opportunities to test different possibilities. This farm with its record of yields, income, and costs is in reality a pilot plant operation. It gives small cotton farmers a proven system which they can follow with whatever modifications may be appropriate in making their own conversions to a diversified enterprise.

The Test Demonstration Program of the Tennessee Valley Authority is another plan to carry research results to the farmers of a community in a form that they can transfer to their own farms. A farmer in each community chosen by his neighbors enters into an agreement to carry out certain cropping and fertilizer practices, keeping records which are available to the cooperating State College and Agricultural Experiment Station, the Tennessee Valley Authority, and his neighbors, and permitting his farm to serve as a local demonstration. In return for this service, he receives the necessary phosphate fertilizer from the Tennessee Valley Authority and advisory help from the Extension Service. The weakness in this plan lies in the fact that it depends on the farmer and a county agent, all-to-often not as well qualified in the soil field as desirable, to synthesize the separate elements of crop and soil science into a farm plan. The plan also necessarily reflects what the farmer thinks he can undertake. Plans developed under such circumstances are obviously far from ideal. On the other hand it is highly significant that in the area served by the Test Demonstration Program, the use of phosphates (purchased by farmers above the amounts provided by the Tennessee Valley Authority), the acreage of perennial legumes, and the entire general level of farm operations have all gone up to a marked degree.

Others have proposed setting up in each state Pilot Farms on which soil management systems indicated by soil research as probably feasible could be evaluated in economic terms on different soil types and for type of farming factors. As a next step in this plan, community farms similar to the Test Demonstration Farms of the TVA plan would carry the proven findings of the Pilot Farms direct to individual communities. Farmers could then choose whatever system for the type of farming their own interest and circumstance might dictate.

Another system that seems in many ways to offer promise is an individual farm advisory service similar to that provided by the Soil Conservation District or the Farm Home Administration. This plan would provide in a county or district for well trained technicians competent to work with

each farmer desiring the service in developing a well rounded farm plan for his farm. Such a plan to be adequate should cover field layout and farm fences, crops (including varieties), crop rotations, land drainage, liming, fertilizers, tillage schedules for preparing soil in cropping and for weed control and water conservation, erosion control practices, manure conservation and use, timing of all operations, choice and effective use of equipment, numbers and kinds of livestock, animal breeding, feeding, and health, preparing and storing feed and other crop materials, housing livestock, facilities for farm processing, crop insect and disease control, and any other pertinent factor. This may seem like a large order, but it is the framework within which sound soil management must be developed. It likewise provides for the kind of integrated educational program needed to implement all agricultural research effectively.

The Soil Conservation Service has been circumscribed in what it can do under the above type of operation. Forced to emphasize only one phase of the problem, even when including factors indirectly contributing to erosion control, they have not been able to cover the ground adequately. The soundness of the over-all idea is shown, however, by the encouraging progress which they have made, limited as they have been by being forced to operate on only one phase of the larger field. The FHA, likewise, dealing with a less well equipped group has shown favorable progress. The success of commercial farm management advisory services further demonstrates the advantages of the method as a way to implement farm science and to teach farmers. Many of the commercial operators have increased farm income for their clients from 50 to 100 percent and at the same time have greatly improved the soil as a producer and a permanent investment.

No one would wish to discredit in any way the fine work so far done in transmitting many phases of agricultural research to farmers. When it is so clearly evident, however, that the present system has not proven entirely adequate to care for one of the most important factors in all farm operations, we should give serious consideration to developing the kind of set up that will do the job. A technological service organized on a district or county basis, to provide advice and help to each individual farmer in developing his farm plan seems to be a workable method. It would provide a most effective means to implement crop, soil, economics, and other phases of agricultural science, and make it possible to reach large and small farmers alike.

The problem of implementing the findings of soil science, *e.g.*, on farms in the United States has been pointed out. When difficulties of this magnitude exist in this country with its high level of literacy, it is evident that in less fortunate countries even more emphasis must be placed on effective methods of farmer education if science is to make any significant contribution. This is one of the most pressing obligations of science in a worldwide program of "Freedom from Want."

Failure properly to bridge the gap between primary soil research and farm use is not the only sin of omission on the part of science in developing sound soil use. There has been the same wide gap between original development of disease and cold-resistant alfalfas and clovers, essential in a sound soil program, and the commercial production of adequate seed of these legumes. This is the principal reason why these crops have not

increased in acreage as they should. During the past 30 years alfalfa wilt and clover anthracnose, together with other diseases and insect pests, have increased in prevalence and severity. No common variety of alfalfa can maintain a satisfactory stand for longer than three years in most locations, and common varieties of red clover may not last even two years. Coupled with this situation has been a marked reduction in seed yields of these crops due to insect pests, lack of pollinating insects, and other factors, with resultant higher costs. The combination of high cost to establish, and uncertain life of stand has discouraged production. And this in spite of the fact that farmers are well aware of the advantages of producing their protein feed in the form of cheap roughage, and the value of these crops in the soil building and maintenance program.

Science has made a good start towards meeting this problem by developing the wilt resistant Ranger and Buffalo alfalfas, and the anthracnose resistant Cumberland and Midland clovers. Among them these varieties cover requirements for the major areas of the country. Unfortunately, so far as most farmers are concerned, there is only a very small amount of seed available for farm use. Cumberland clover has now been out for some eight years and yet only 385,000 pounds of seed of this variety were certified in the Western seed producing areas in 1946. There should be 40,000,000 pounds available each year. The story for Ranger and Buffalo alfalfas is slightly better, but is not enough so for much satisfaction. There is available a mere 1,000,000 pounds plus of seed where there should be 60,000,000 to 75,000,000 pounds. Some of the recent difficulty has been due to competition with food crops during the war and improvement in this regard can now be expected. The real trouble, however, has been failure to produce sufficient quantities of foundation seed stocks. The improvement job was dropped at too early a phase of its progress from the breeder to the using farmer. Fortunately steps are being taken to correct this situation, but we are several years behind where we should be because all responsibilities that go with the over-all job were not originally recognized and met. Science has the obligation to complete its job to the point where its findings can be fully used.

One of the most critical problems in the long time food program faces us in the great central and western wheat-producing regions of this country. This is the real bread basket of the land. The problem which we face there is common to other great grain producing regions of the world in Europe, Asia, Africa, South America, and Australia. Data taken from rotation and soil management plots show that under cropping there is occurring a gradual reduction in soil organic matter and in soil nutrients, particularly nitrogen. So far no practicable crop or cropping system has been found for restoring either the depleted nitrogen or organic material. As the depletion process continues, the physical relations of the soil with respect to moisture penetration and retention and wind and water erosion are certain to be serious. The nitrogen phases of the problem seem less acute than the organic matter, since they can be corrected by adding commercial fertilizer. It may be that corrective measures for organic matter maintenance can be worked out by using commercial fertilizers in combination with available crop residues and grass rotations, but we do not know. The problem is a serious threat to ultimate world food supply. There is a most pressing obligation on science to attack it with vigor.

Previous discussion, without specifically noting the fact, has emphasized the close and intimate relationship of economic and allied factors to the application of science in agriculture. Every step that the agricultural scientist takes in using science on farms must be sound economically to succeed. This does not mean that the scientist should undertake to work out all economic factors himself. It is rather his obligation to work with the economist to integrate science and economic relationships.

It is obvious that "Freedom from Want" can be attained only by attack on a very wide front. It is not alone a question of the natural sciences and their ability to develop means to provide food. Religion, superstition, racial customs and prejudices, economics, politics, to name only a few factors, play a part. To the extent that the social sciences can contribute in solving these factors in the complex, they must be recognized and used.

Modern science is highly specialized. Specialists, unless keenly conscious of the broad implications of their own contributions in relation to other factors, may not always be aware of the part which other disciplines must play and are playing in making their own results effective. It is always an unfortunate human trait that an individual who succeeds notably in one field may be inclined incorrectly to assume equal competency in another. Scientists must guard against such attitudes.

During the war an article appeared in a leading scientific journal authored by the head of an important government laboratory attached to one of the armed services. This article discussed at some length the part that various government agencies were playing in effective war research, stating, however, that with the exception of certain named minor activities, the Department of Agriculture was not entering into war research to any great extent. All those with full knowledge of the facts recognize that the food production research of the Department of Agriculture alone contributed as much as any other single item to the war effort. And this includes the atomic bomb, for without the food research in all likelihood there would have been no time for atomic research to come to fruition. The statement in terms of the author's concept of war research was undoubtedly technically correct. In terms of the necessary integration of all scientific effort in winning the war, it did not take all factors into account. The incident is not important but it does forcefully emphasize the need for a full appreciation of the part which contributions from various fields of science may play in the over-all solution of any social or political problem. It emphasizes an obligation to maintain contacts and to understand all contributing relationships of the natural and social sciences in solving the immensely difficult problem of "Freedom from Want."

Finally, as scientists, we must remember that the solution of any problem in science is not alone dependent on accumulating and analyzing facts according to the experimental method. It is also dependent on an open and inquiring mind and all that this implies in clear sound thinking. "An open and inquiring mind" defines in simple summary our primary obligation in attacking the problem of "Freedom from Want."

The AUTHORS

HOWARD ROSS TOLLEY was born in Howard County, Indiana, in 1889. Graduated from Indiana University in 1910 he was for a year a member of the U. S. Coast and Geodetic Survey. He then entered the field of agricultural economics where he rose rapidly to Senior Agricultural Economist in the U. S. Department of Agriculture in 1928. Since that time he has held many important posts, including among others, Professor of Agricultural Economics, University of California; Agricultural Economist and Director of the Giannini Foundation, consulting specialist, Agricultural Adjustment Administration, U. S. Department of Agriculture, 1933, and from Chief Economist in 1933-34 to Chief of the Bureau of Agricultural Economics, U. S. Department of Agriculture in 1938. He continued in this position until the Food and Agriculture Organization of the United Nations was created. In this Organization he was selected to direct the section on economics and statistics. Dr. TOLLEY is a member of various scientific societies including the Economics Association, the Farm Economics Association and the Statistical Association.

ROBERT MUNDHENK SALTER was born in Huntington, Indiana, March 13, 1892. After earning the B.S. and M.S. degrees at the Ohio State University, he joined the staff of the West Virginia Agricultural Experiment Station, where he advanced through the ranks to Professor of Agronomy in 1919. In 1921 he transferred to Ohio State University and was soon advanced to Chief in Agronomy which position he held until 1940, being chairman of the department from 1929. Then, after one year's service as Director of the North Carolina Agricultural Experiment Station he became Head of the Division of Soil and Fertilizer Investigations in the Bureau of Plant Industry, Soils and Agricultural Engineering of the U. S. Department of Agriculture. A year later he became Chief of the Bureau. Dr. SALTER is recognized as one of the leading authorities in this country in the field of soil fertility, management and conservation, and has contributed greatly to the advancement of that field by his research and publications, and, in his present capacity by his leadership in the design and prosecution of research projects throughout the United States. He is a member of the American Society of Agronomy having been its president in 1936 and is also a member of the International Soil Science Society.

KARL SPANGLER QUISENBERRY was born in Denison, Texas, August 2, 1897. He was graduated from the Kansas State College in 1921 and received M.S. and Ph.D. degrees from the University of Minnesota in 1925 and 1930. Dr. QUISENBERRY served as instructor in the Agronomy Department of the University of West Virginia from 1921 to 1925. He then joined the staff of the Bureau of Plant Industry, Soils and Agricultural Engineering of the U. S. Department of Agriculture and advanced through succeeding ranks to Senior Agronomist in 1942, and to the headship of the Division of Cereal Crops and Diseases in 1946. While stationed in Nebraska (1936-1946) he served the State at the same time as Professor of Agronomy in the University of Nebraska. His field of special interest is plant breeding and he has made noteworthy contributions in the genetics and improvement of wheat and in the development of varieties combining winterhardiness, yielding ability, and grain quality.

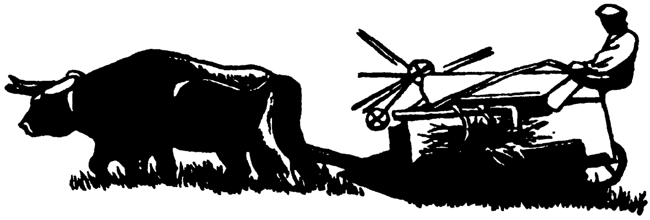
Born at Fort Atkinson, Wisconsin in 1887 FRANK BARRON MORRISON came from Scotch-English ancestors who had settled in colonial New England. After growing up on a dairy farm he taught a district school following high school and then earned his way through the University of Wisconsin by helping W. A. HENRY revise his classic "Feeds and Feeding." In more recent years he has entirely rewritten the book under his own authorship, and is now completing another revision which will be the twenty-first edition. On graduation in 1911 he immediately became assistant to the Director of the Wisconsin Agricultural Experiment Station. During the next sixteen years he enjoyed a wide experience in the College and Experiment Station, doing teaching and research in the Department of Agricultural Chemistry, later in Animal Husbandry, and serving as Assistant Director and at various times as Acting Dean and Director. He reached the rank of Professor of Animal Husbandry in 1919. In 1927 he moved to Ithaca, New York, as Director of the Experiment Station at Geneva and Ithaca, associated with Cornell University. A year later he was asked to become head of the Animal Husbandry Department at Cornell University to reorganize and expand the Department of Animal Husbandry, a work which resulted in the acquisition of adequate farms, new buildings, foundation pure bred herds of the various breeds, and an adequate teaching and research program. He has served on commissions for reorganizing the livestock industries of Germany and the Philippines, and has conducted special courses in several State Colleges of Agriculture in the United States. He has also been called upon to address agricultural and scientific organizations in many states as well as in Canada, Hawaii, the Philippines, England, and India. He is a member of the scientific societies representing his field of research in this country and abroad, is a Fellow of the American Association for the Advancement of Science and past president of the American Society of Animal Production.

JOHN DONALD BLACK was born in a log cabin on a homestead in Jefferson County, Wisconsin, twenty-five miles from Madison. He graduated from the Fort Atkinson, Wisconsin, High School in 1899, from the University of Wisconsin in 1909, majoring in English, and received his Ph.D. from the University of Wisconsin in 1919, majoring in the economics of agriculture. He became head of the Div. of Agricultural Economics of the University of Minnesota in 1921. By 1927 he had developed almost the largest group of graduate students in agricultural economics in the country and had published in 1927 the pioneer treatise in the English language on the economics of production, a general treatise that includes industry as well as agriculture. Since 1929 Dr. BLACK has taught and directed research in the Department of Economics of Harvard University. From 1929 to 1932 he was Chairman of Commission I on Food Supply of the International Population Union. In 1929 he was author of *Production Organization* and of *Agricultural Reform in the United States*. His two latest are *Farm Management*, published in 1947, and *Food and Agriculture Policy*, now in press. During the war he published *Parity, Parity, Parity* and *Food Enough*. He was Chairman of the Advisory Committee on Agriculture of the Social Science Research Council from 1929 to 1935 and Director of its project on Scope and Method of Research in Rural Social Sciences. He has been consultant for branches of the Government at intervals since 1922, including the Department of Agriculture, Federal Farm Board, the TVA and the Farm Credit Administration.

MAX ADAMS MCCALL (B.S. Oregon State '10; M.S. Washington State '22; Ph.D. Wisconsin '32) was born in 1888 in Jamestown, Kansas. After graduation from the Oregon State College he engaged in high school teaching and county agricultural agent work in Oregon and Washington, and then began a 10-year period of service in the Experiment Station of Washington as a dry land specialist and branch station superintendent. Entering the Federal service in 1924 in charge of cereal crop agronomy in the Cereal Division of the Bureau of Plant Industry, he has advanced from Agronomist to Head of the Division and now Assistant Chief of the Bureau. Coupled with his own advancement was the important rôle he has played as an administrator during the period of development for combine type grain sorghums, hybrid corn, and disease-resistant varieties of wheat, oats, and other cereals, all of which have reached fruition on

American farms. He is a member of numerous professional organizations, including the American Society of Agronomy, of which he was President in 1932, the American Association for the Advancement of Science, of which he is a Fellow, and Vice-President for Agriculture for 1947, and the Amercian Society of Plant Physiologists.

Ernest E. DeTurk (B.S.A. Purdue '13; M.S. Pennsylvania State College '16; Ph.D. Illinois '19) was born in 1887 and grew up on a farm near Martinsville, Indiana. Following high school he taught a one-room country school for three years. On leaving Purdue University he was on the teaching staff of the Department of Agricultural Chemistry at Pennsylvania State College from 1913 to 1917, advancing from Assistant to Assistant Professor. Following a Fellowship in soils at the University of Illinois he joined the staff of the Agronomy Department, where he has been Head of the Soil Fertility Division since 1920. Together with his associates he has contributed by research, teaching, and publications to the understanding of the chemical aspects of soil development; the chemical nature of soil potassium, phosphorus, and calcium, and of added forms of these elements as they influence the nutrient supplying power of soils for growing crops; and the chemical and physiological basis of the development of nutrient deficiency symptoms in plants. He is a member of the American Society of Agronomy, Soil Science Society of America (Secretary of the Chemistry Section, 1937 and Chairman, 1938), American Chemical Society, a Fellow of the American Association for the Advancement of Science, and Secretary of the Section on Agriculture, 1945-date, and other professional organizations. He is also a consulting editor of Soil Science.



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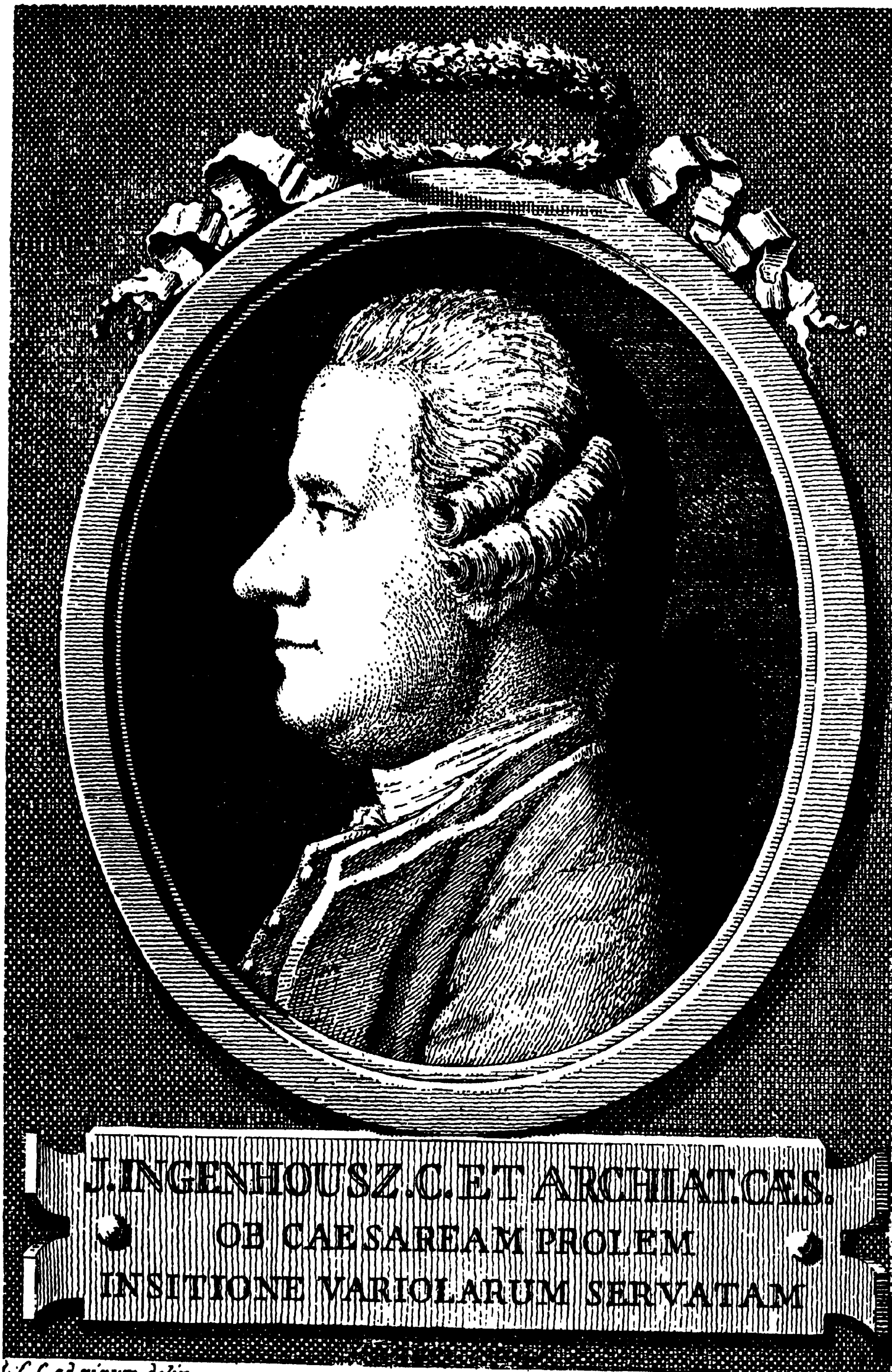
INGENHOUSZ

Plant Physiologist



JAN INGENHOUSZ. — Engraving by CUNEGO (Rome, 1769) after a drawing, 'ad vivum', by A.L.L. — At the bottom INGENHOUSZ' motto *utinam citius*. The lines *ob Caesaream prolem insitione variolarum servatam* refer to the successful inoculation of the children of Empress Maria Theresia's household. The 'Prentenkabinet' at the Hague owns a smaller lithograph, by H. J. BACKER (?), after A.L.L.'s portrait. (*From* the original in the Chronica Botanica Archives).

JAN INGENHOUSZ

Plant Physiologist

With a History
of
the Discovery of

PHOTOSYNTHESIS

by

HOWARD S. REED, Ph. D.

Professor of Plant Physiology, Emeritus, University of California

Author of 'A Short History of the Plant Sciences'

"And that thou mayst marvel less at my words, look at the sun's heat, that is made wine when combined with the juice which flows from the vine."—DANTE, Purgatorio XXV, vv. 77-78 (Carlyle-Wicksteed transl., Modern Reader's Libr., New York, 1932).

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Vignettes by Hilde de Vries.

FOREWORD BY THE ANNOTATOR

INGENHOUSZ *was one of the founders of the science of plant physiology whose fruitful researches in the late eighteenth century stimulated others to attack some of the most abstruse problems of the activities of organic life. His discovery of the action of light on the green cells of plants and his appreciation of its importance in the economy of nature mark him as a discerning genius. The experimental work upon which his epoch-making discoveries rested was accomplished with little equipment and with a few crude chemicals. Today, with cunningly devised apparatus, and in elaborately equipped laboratories, men still delve into problems which engaged* INGENHOUSZ. *Much has been learned about photosynthesis since his day, but the end of the research is not yet in sight.*

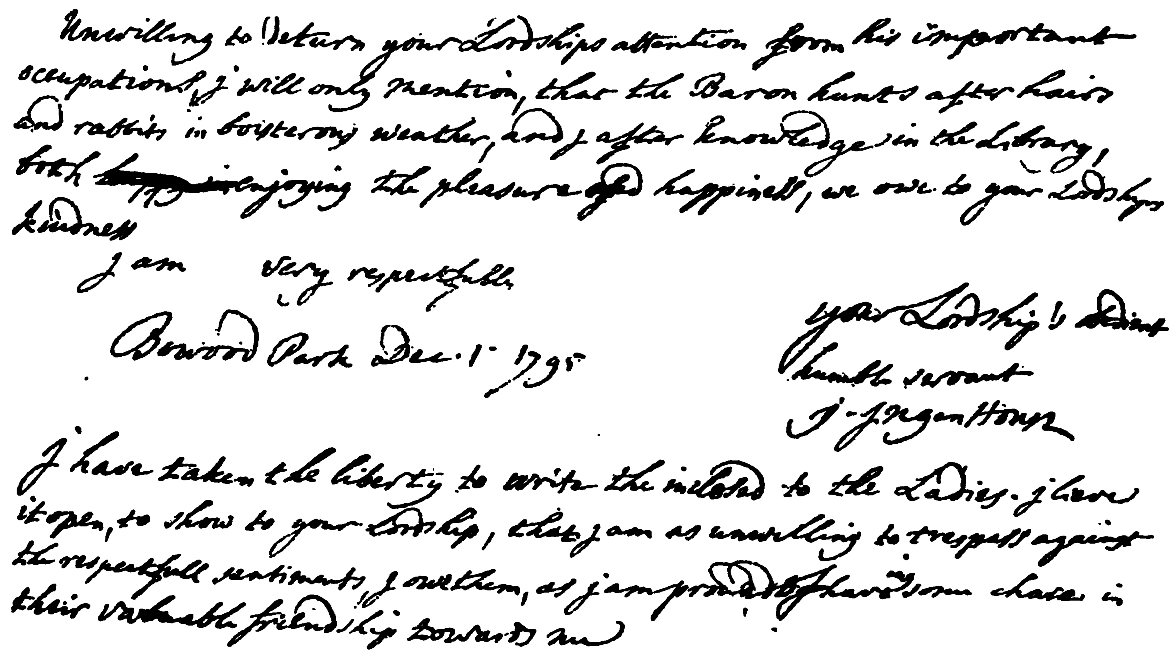
The classical work of INGENHOUSZ, *which marked such an important advance in the development of scientific ideas in the eighteenth century, has long been out of print, and is generally known only through second-hand quotation. The annotator and the editor of* CHRONICA BOTANICA *have undertaken to issue the present edition because they feel that many of* INGENHOUSZ' *discoveries motivated scientific work which is still productive. The text of the English edition of 1779, with a few interpolations from the French edition of 1787, is herewith reproduced. The comments which follow many of the sections of the original text are offered in the hope that they may guide those readers who wish to trace the influence of* INGENHOUSZ' *work on various developments in modern research. The annotator is aware that he has written on subjects which particularly interested him. The specialist who wishes a modern, well-rounded discussion of the phenomenon of photosynthesis will not find it here. The protocol of the numerous experiments which occupies considerable space in the original book is not included.*

A brief biographical sketch shows how the work of INGENHOUSZ *fitted into the scientific awakening of his time. The opportunities which* INGENHOUSZ *had for travel and discussion with men of his time undoubtedly did much to develop his ideas on the subjects in which he was interested. A man's work is always more or less a product of the times in which he lives. I have, therefore, introduced some reflections on* INGENHOUSZ' *contemporaries and the conditions of science in which they worked. One should not forget that the experimental sciences were then in their infancy. The mortality rate of the newborn sciences was high. Even the lot of those which survived was unhappy, subjected, as they often were, to bitter criticism, suspicion, and hatred. There were few men at that time who ventured to explore the inner functions of plants and interpret some of the processes which are responsible for phenomena which had been previously cloaked in mysticism and sophistry.*

Among other writers whose works have afforded historiographic material, I must mention TREUB (1880), WIESNER (1905), *and* TRÖNDLE (1925). *As far as possible, I have attempted to give the credit due various authors in the lists of titles at the end of each section, but there may be some which have not been properly acknowledged. If so, I apologize for the oversight.*

I am especially indebted to Professor H. A. BARKER *for many helpful suggestions and advice.*

Several of the illustrations and plates were secured by Dr. VERDOORN from the *Netherlands — in this connection we are particularly indebted to Dr.* M. ROOSEBOOM, *Rijksmuseum voor de Geschiedenis der Natuurwetenschappen, Leiden, to Professor* J. A. VOLGRAFF, *University of Leiden, and Dr.* P. SCHERFT *of the Breda Municipal Archives. There are many valuable* INGENHOUSZIANA *at Breda, including an unpublished diary, letters, a prescription booklet, a herbarium with note book, as well as several unpublished manuscripts — all material well worth further study by a Dutch historian of science. Dr.* VERDOORN *also tried to secure a photograph of* INGENHOUSZ' *house in Breda (which no longer exists in its former state—it is now a store), after a note had been found by Dr.* VOLGRAFF *according to which the house had been photographed in 1868 by Messrs.* KAMERMANS AND SONS (?). *This photograph could not be located according to Ir.* A. H. INGEN-HOUSZ (*Beverwijk*), *a direct descendant of* JAN INGENHOUSZ' *brother, to whom we are also obliged for help and advice.*



Unwilling to detarn your Lordships attention from his important occupations, I will only mention, that the Baron hunts after hairs and rabbits in boisterous weather, and I after knowledge in the Library, both ~~happy~~ enjoying the pleasure and happiness, we owe to your Lordships kindness

I am very respectfully

Bowood Park Dec. 1. 1795

your Lordship's obedient
humble servant
J. Ingen Housz

I have taken the liberty to write the inclosed to the Ladies. I leave it open, to show to your Lordship, that I am as unwilling to trespass against the respectfull sentiments I owe them, as I am proud of having some share in their valuable friendship towards me

AUTOGRAPH (END OF A LETTER) BY JAN INGENHOUSZ (*from* the original in the Chronica Botanica Archives).

BIOGRAPHICAL SKETCH

JAN INGENHOUSZ was born December 8, 1730 in Breda, in North Brabant, and died September 7, 1799 at Bowood Park, near London. His father, ARNOLD INGENHOUSZ, was a prominent citizen and successful business man of Breda. Young JAN studied in Breda and in Louvain, received the degree of doctor of medicine at the age of twenty-two, studying subsequently at Leiden, Paris and Edinburgh. In 1757 he returned to his native city and began the practice of medicine in which he was so successful and had so many patients that soon he had no time for researches in physics or chemistry, except late at night. It is said that his love for his father was the tie which kept him in Breda and decided him to forego for the time the richer scientific opportunities abroad. During the time he practiced in Breda he nevertheless kept up correspondence with his former teachers in Holland, with Sir JOHN PRINGLE and others in England.

Soon after the death of his father, JAN INGENHOUSZ took up his residence in London and continued the practice of medicine. According to TREUB (1880) this move occurred in 1766. He considered his improvements in the art of inoculation for smallpox as one of the principal achievements of his medical career. In many ways the personality of INGENHOUSZ was the counterpart of that of JOSEPH PRIESTLEY, who was also a pioneer in the search for truth, but a proponent of radical ideas in religion and politics. INGENHOUSZ came from an affluent family, was well educated, a favorite of royalty and a conformist in social and religious matters.

A few words are appropriate concerning PRINGLE to whom INGENHOUSZ acknowledged his indebtedness in the foreword to his book on Experiments upon Vegetables.

Sir JOHN PRINGLE was born August 10, 1707 at Stichel House in Roxburghshire in the north of England. He was educated at St. Andrew's, at Edinburgh, at Leiden and at Paris. He was called in 1734 to Edinburgh University where he was appointed professor of metaphysics and moral philosophy. His preference for the field of medicine led him to relinquish his chair at Edinburgh and to establish himself in London in 1748 after a brief service as surgeon in the British army in Flanders and Germany. He left the army service and, upon the accession of GEORGE III, was appointed physician in ordinary to their majesties.

PRINGLE was elected a member of the Royal society in 1753 and became its president in 1772. He died in London in 1782 and was buried with honors in Westminster Abbey. His greatest contribution to medicine was his work on army sanitation. His book "Observations on the Diseases of the Army," first published in 1752, went through numerous editions.

Whilst PRINGLE was with the British army on its campaigns in Holland he frequently visited the home of INGENHOUSZ, one of the esteemed burgers of Breda, and was attracted to JAN, the older son, who at that time was attending the Latin school. The extraordinary ability and the amiable character of the youth made such a favorable impression on PRINGLE that it started a life-long attachment. INGENHOUSZ' dedication of this work to

Sir JOHN PRINGLE expressed in no empty phrases his homage to the man who gave him the greatest inspiration he had received. In London, Pringle introduced him to such physicians as WILLIAM HUNTER, GEORGE ARMSTRONG and ALEXANDER MUNRO.

INGENHOUSZ' success in the art of inoculation for the treatment of smallpox was responsible for his call to Vienna where an unusually severe epidemic of the disease was raging. Several members of the Royal family had died of the disease. Empress MARIA THERESA, impressed by the success of inoculation in England, decided to introduce it into Austria and to attempt to save the lives of her remaining children. Prince KAUNITZ commissioned, in the name of the Empress, Count SEILER, the Austrian ambassador in London, to get the most suitable person in England for this purpose. PRINGLE proposed INGENHOUSZ. The King approved the nomination and the intention was reported by the Austrian ambassador to Vienna. Upon his arrival, May 14, 1768, at Vienna INGENHOUSZ was met by a high court official who showed him proper honors and took him to an apartment in the castle. He remained in Vienna until 1779, achieving great success in immunizing patients by inoculation, then returned to London, where he resumed his studies on the relations between air and plants (*cf.* WIESNER, 1905).

Many have wondered, no doubt, why INGENHOUSZ went back to England after having lived for several years in Vienna. From a knowledge of his surroundings and from statements in his letters, one seems justified in concluding that he returned to England because he did not find in Vienna the continuous time and the composure which were necessary for such a large project as the study of the gas interchange in plants. His relations to the court, to the aristocracy, and to Viennese society in general, were so manifold and his duties so numerous that he found little rest or independence of action. Furthermore, he was no doubt inclined to hope that in London he could obtain the necessary instruments and apparatus of better quality than he could get in Vienna. He also planned from the outset to issue his work in English. He was elected a Fellow of the Royal Society in 1779. His genial manner and social graces made him a welcome guest at English country houses; WILLIAM, Earl of SHELBURNE, welcomed him to his residence, Bowood, and gave him a room which was long known as "The laboratory." FITZMAURICE wrote (1876) that when INGENHOUSZ felt that he would not long survive he planned to leave Bowood for fear of giving trouble in his last illness. SHELBURNE induced him, however, to remain and end his days there. When JENNER discovered the immunizing power of cow-pox and introduced the practice of vaccination, INGENHOUSZ could not accept it, but confined his opposition to a private letter, declining to be drawn into open scientific controversy. SHELBURNE used to say that he always believed BENTHAM to be the most good-natured man in the world until he met INGENHOUSZ.

INGENHOUSZ left no doubt about the way in which his investigation on the gas exchange of plants was initiated. He expressed himself with clarity, telling not only the time at which his plan of studying this important phenomenon was formulated, but also acknowledging the beautiful relationship which he bore to PRIESTLEY, by whom he was later treated in the most unjustifiable manner. He repeatedly said that he studied the problem of

gas exchange many years before he began to write. His colleague, SCHERER, said that the preparation for the work dated from 1773. We are certain that he devoted considerable attention to the problem while he was in Vienna. The chemist, SCHEELE, engaged in a study of the relations between plants and the atmosphere, but, not appreciating the role of light, got results which were completely opposed to those obtained by PRIESTLEY. The disagreement was successfully explained by INGENHOUSZ. Shortly thereafter both PRIESTLEY and SCHEELE abandoned work on the problem. When INGENHOUSZ took it up he managed the experiments more successfully. He did not belittle PRIESTLEY's discovery in which truth and error were curiously entangled, but came upon the right road after his preliminary experiment in which he demonstrated the importance of the action of light.

INGENHOUSZ formulated five elemental statements concerning the nutrition and gas formation by plants:

1) Growth does not condition the oxygen evolution by plants.
2) Oxygen evolution is most vigorous in sunlight, but is deficient in diffused daylight.
3) Plants differ in this respect. Some even liberate oxygen for a time after sunset, also in twilight.
4) Oxygen evolution not only stops in darkness, but there is a vitiation of the air which is harmful for animals.
5) The dephlogisticated air (oxygen) streams principally from the lower side of the leaf. Young, immature leaves exhale less pure dephlogisticated air than fully grown leaves. Some plants, especially certain aquatics, are peculiarly adapted for the evaporation of this air.

In the light of these statements the disagreement between PRIESTLEY and SCHEELE can be understood. PRIESTLEY could not differentiate the two processes of gas evolution. SCHEELE had apparently observed only one of these processes because his plants were kept in light which was too weak for oxygen evolution.

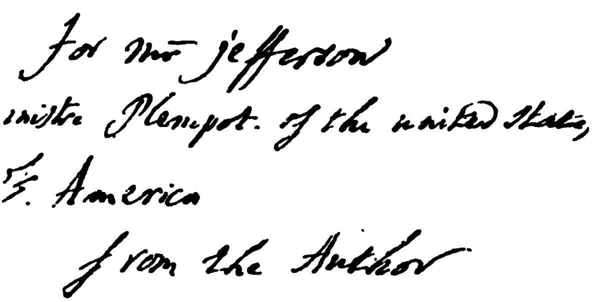
The rapid advances in the field of electricity claimed INGENHOUSZ' attention and inspired him to make researches. He delivered a lecture before the Royal Society (1778) in which he discussed specifically FRANKLIN's theory of positive and negative electricity. If his explanations seem somewhat disingenuous to us, we must nevertheless admit the soundness of much of his basic philosophy.

> "A conducting body insulated, being placed in the sphere of action of an excited, non-conducting body, or even in contact with it, acquires at the same time two contrary electricities; viz., the part in contact, or very near the non-conducting electrified body, acquires a contrary electricity to that of the non-conducting body, at the same time that the opposite or farthermost extremity is possessed of the same electricity with the conducting body."

INGENHOUSZ delivered (1779) another lecture before the Royal Society on "Improvements in Electricity," describing his inventions of improved apparatus to generate electricity by friction. Having observed certain defects in the glass plates used at that time, he devised a machine which consisted of large pasteboard disks covered with a varnish of linseed oil and amber. Several disks of this material mounted in a strong wooden frame generated electricity strong enough to produce sparks one to two feet long from the surface of the first disk.

He was also interested in the applications of electricity to practical affairs, demonstrated by his studies of the best kind of tips for lightning rods. His genial relations to FRANKLIN are reflected in the letters reproduced on pages 391 and 392 of this book (*see also* PLATE 65 and *infra*).

Of the versatility and awareness of INGENHOUSZ' mind we shall not speak further, leaving the reader to gather his own impressions from the text and comments which are spread on the following pages. He won for himself an eminent position in the great advance in the latter part of the eighteenth century from which there has been no retreat.


for mr jefferson
ministre Plenipot. of the united states
of America
from the Author

FACSIMILE OF INGENHOUSZ' INSCRIPTION TO THOMAS JEFFERSON IN A COPY OF THE FRENCH EDITION, NOW IN THE LIBRARY OF CONGRESS IN WASHINGTON, D.C.

CHEMICAL STUDIES WHICH LED TO THE DISCOVERY OF PHOTOSYNTHESIS

It would be futile to pretend that the work of INGENHOUSZ sprang independently from the earth and bore fruit without the fertilizing influences of preceding workers in science. Fundamental to much that took place in the last of the eighteenth century, we must recognize the resurrection of the atomic theory, which was originally conceived by DEMOCRITUS and EPICURUS. The modern development of the atomic theory stems from the work of BOYLE, although he never made any dogmatic assertions concerning the validity of the hypothesis of DEMOCRITUS and EPICURUS. Nevertheless, he considered atoms or corpuscles to be a constant element of matter. He realized that these small particles, each different from the other, were often immediately compounded into perfectly mixed bodies. Neither BOYLE nor his contemporaries ventured to assert that any known substance was such an element, and the subsequent rise and acceptance of the phlogiston theory retarded the recognition of the elementary character of metals and bases. This was a long advance from the old-time hypothesis that matter was composed of four elements: earth, air, fire and water. It is easy to trace the influence of BOYLE's conclusions concerning the physical nature of matter upon HALES.

STEPHEN HALES published (1727) his views on the food of plants fifty years before the announcement by INGENHOUSZ on the subject. Sometimes he reasoned erroneously, but he was certain that plants derive an important part of their nutriments from the air. For example, he saw bubbles of gas rise and form froth on the sap which exuded from the stumps of decapitated vines on warm days and concluded that the bubbles rose from air which was drawn in through the roots and stem.

Readers often overlook HALES' extensive analyses of the air and the demonstration that the gases liberated by dry distillation could have come only from the animal, vegetable, and mineral substances he employed. In many ways he was PRIESTLEY's forerunner. He placed oak wood in a closed container and applied heat, obtaining a definite amount of gas, which he tested for its properties. He said, "Eleven days after this Air was made, I put a live Sparrow into it, which died instantly." He made dry distillations of several other vegetable substances and determined the amounts of gas obtained.

Experimenting on the absorption and emission of air from plants, HALES concluded, "Whence it is very probable, that the air freely enters plants, not only with the principal fund of nourishment by the roots, but also thro' the surface of their trunks and leaves, especially at night, when they are changed from a perspiring to a strongly imbibing state" (pg. 153).

HALES' concept of gas interchanges between a plant and its environment opened the way which others entered many years later. PRIESTLEY was one of the first to appraise (1774-77 and later editions) his discoveries, in the following words:

"Dr. HALES, without seeming to imagine that there was an immaterial difference between these kinds of air and common air, observed that certain substances and operations *generate* air, and others absorb it; imagining that the diminution of air was simply a taking away from the common mass, without any alteration in the properties of what remained. His experiments, however, are so numerous, and various, that they are justly esteemed to be the solid foundation of all our knowledge of this subject."

"Mr. CAVENDISH had exactly ascertained the specific gravities of fixed and inflammable air, shewing the former of them to be 1½ heavier than common air, and the latter ten times lighter. He also shewed that water would imbibe more than its own bulk of fixed air" (Experiments and observations on different kinds of air. Vol. 1, pg. 2).

Lest anyone should think that HALES misapprehended the nature of organic matter from some of his naive statements, he should read the conclusion on pg. 210. Having described the volumes of gases evolved by incineration, or by fermentation, he concluded that the active particles of the gas were not compressed in the organic matter, but were in a fixed state "before they were roused, and put into an active repelling state by fermentation and fire."

HALES emphasized his conclusions on the role of gases in forming an integral part of plants on p. 318 though he continued to use the antiquated word "air." Recognizing that plants contain sulfur, volatile salts, water and minerals, he added that "air" unites those elements and thereby forms compounds which serve as the appropriate nutriment of plants (p. 323). HALES' work on transpiration gave him incontrovertible evidence that leaves are instrumental in raising the sap stream and transmitting water and gases to the atmosphere, but he also propounded the idea that leaves may acquire some essential part of their nourishment from the air. INGENHOUSZ stated in the preface to his book that the purification of the air serves the plants as some kind of nourishment, but he did not pick up the thread where HALES dropped it.

HALES anticipated INGENHOUSZ in the theory that evergreens which retain their leaves during the winter have a viscid, oily sap, the better to endure the winter's cold. Noting that they flourish most in the temperate seasons of the year, he concluded that the slower movement of sap was unable to supply enough water to meet the transpirational losses in hot weather.

Finally, HALES considered the action of light on the leaves and flowers as an agent in forming the substance of plants, referring to NEWTON's query on the activating power of the corpuscles of light.

When INGENHOUSZ came into England, he found a scientific group which believed most firmly in the old doctrine of phlogiston. Without any definite evidence to support such a theory, it was held nevertheless by the majority of physicists and chemists of the day. The word was introduced by the German doctor, STAHL.

Phlogiston was believed to be the principle of combustibility. It was supposed that when a substance burned, or a metal was calcined, it was decomposed, the phlogiston passing off into the air and the ash remaining. The less ash there was, the nearer did the substance approach to pure phlogiston.

Chemists held different ideas as to the nature of phlogiston. *1*. BEAUMÉ—the matter of fire united to elementary earth. *2*. MACQUER—the matter of light in a fixed state. *3*. SCHEELE—a constituent part of the matter of heat (the other part being vital air). *4*. VOLTA—fixed air combined with the matter of heat. *5*. KIRWAN—the basis of inflammable air.

The doctrine of phlogiston had no more staunch defender than HENRY CAVENDISH (1731-1810) who upheld the theory even after LAVOISIER had laid the foundation for scientific chemistry. CAVENDISH designated hydrogen as "inflammable air" but identified it with phlogiston in 1766. He combined hydrogen and oxygen some years later by means of the electric spark, and then called hydrogen "inflammable air" and oxygen "dephlogisticated air." He said in his paper published in 1784:

> "I also procured some dephlogisticated air from the leaves of plants in the manner of Drs. INGENHOUSZ and PRIESTLEY and exploded it with inflammable air as before; the condensed liquor still contained acid, and of the nitrous kind."

This paragraph shows that CAVENDISH was acquainted with INGENHOUSZ' work, and we can assume with some degree of assurance that INGENHOUSZ was familiar with the former's work on gases. CAVENDISH mentioned the results of his work on exploding inflammable and dephlogisticated air to PRIESTLEY, who subsequently made similar experiments, and a friend (BLAGDEN ?), communicated them to LAVOISIER who likewise repeated them. The ungenerous attitude of LAVOISIER called forth a strong protest from BLAGDEN on the way in which the results were published without giving credit to CAVENDISH. In this place we need not, however, be concerned with the polemic which ensued. CAVENDISH regarded LAVOISIER'S new method of chemical nomenclature as useless and was loathe to accept it. In fact he berated it in a letter in 1787 to BLAGDEN.

The tenacity with which CAVENDISH clung to antiquated ideas is illustrated by his conclusion that dephlogisticated air (oxygen) is only air deprived of its phlogiston and that inflammable air (hydrogen) is either phlogisticated water or else pure phlogiston, but in all probability the former.

LAVOISIER believed that water is a compound of inflammable air united with dephlogisticated air, but the experiments on which those illustrious physicists based their opinions did not appear sufficiently convincing to CAVENDISH, as one might imagine at the commencement, and the conclusion drawn from them being inexact, according to FONTANA and others, it was necessary to suspend judgment on such important assertions.

When JOSEPH PRIESTLEY discovered that plants liberate a gas which will support combustion, he was impressed with the importance of his discovery, but could not completely interpret it. Had he not been so strongly imbued with the doctrine of phlogiston he might have won for himself a still more eminent place in the development of chemistry.

PRIESTLEY was a master of philosophic reflection on all subjects which he studied. He will ever be remembered for the way in which he used his voice and pen to advance the cause of civil, political and religious liberty as well as to promote scientific knowledge. In pursuit of the latter activity he contrived useful and ingenious experiments, by means of which he opened roads into new fields of chemistry. He worked in an age when old ideas in chemistry were being overturned and new ideas were often in-

definite. The doctrine of phlogiston which had been considered adequate was being seriously questioned as a result of his own discovery of oxygen, but no one appreciated its full significance. PRIESTLEY attempted to rationalize the new discoveries, but eventually returned to the doctrine of phlogiston, feeling that the new theory of chemistry had been too largely supported by ambiguous experimental data (DAVIS, 1927).

LAVOISIER took up the problem where PRIESTLEY quit. Owing to his skill and genius, he was quick to see that the explanation of the fact which had bothered PRIESTLEY could be found if one realized that there was such a thing as chemical affinity and definite combining volume. There is little doubt that LAVOISIER had received from PRIESTLEY personal accounts of his experiment along similar lines and with similar results. Perhaps LAVOISIER failed to give sufficient recognition to PRIESTLEY's work, nevertheless, the memoir which he published was epoch-making in chemical thought, on account of the definite conclusions drawn, in contrast with PRIESTLEY'S somewhat medieval deductions which were published shortly after LAVOISIER's paper. PRIESTLEY, dominated by the phlogiston theory, proposed that the resulting gas obtained from heating iron oxide was common air, partly free from the theoretical phlogiston. LAVOISIER, basing his conclusions on the newer ideas of chemistry, drew a more logical and highly scientific conclusion (HARTOG, 1941). He logically concluded that when metals were heated by glowing charcoal it was through the combination of this last with the pure portion of the air that one obtained what was commonly called "fixed air." He also applied the same reasoning to the action of nitrate and carbon in giving fixed air to show that nitrate must contain "pure air." In this connection he observed:

"Since the carbon disappears completely in the revivication of the calx of mercury and we obtain nothing but fixed air and mercury, we are forced to conclude that the principal to which up to now has been given the name 'fixed air' is the result of the combination of the imminently respirable portion of the air with the carbon." (*Cf.* HARTLEY, 1947).

LAVOISIER made experiments in chemistry in the years 1772 and 1773 in intervals between other duties and concluded that "during burning there is combination with an elastic fluid which, in becoming fixed, is the cause of the increase in weight." His experiments seemed to show that ordinary air is not a simple substance, since only a fifth of it, and never more, played any part in combustion and calcination.

LAVOISIER went so far as to say that in his opinion the future of chemistry was wrapped up in a study of gases and that this would probably result in a revolution in chemistry and physics. Among other conclusions, he stated that combustion occurs only in oxygen (*air éminemment pur*) resulting in the formation of an acid, though metals are calcined. Especially significant is his observation that combustion uses oxygen and that the gain in weight of the substance burned is equal to the loss of weight shown by the air. His statement (OEUVRES, 1792, II, p. 104) that he had conceived the whole doctrine of combustion from the year 1772 may be a claim for originality, but it is not for us to detract from the credit he justly deserves. His observations on the products of the combustion of phosphorus led him to conclude that "Atmospheric air consists of about one quarter of dephlogisticated, or eminently respirable air, and three quarters of a mephi-

tic or noxious air, a kind of gas of unknown nature." Although the proportions of oxygen to nitrogen first stated were incorrect, he made other determinations later which more nearly approximated the correct value.

The results of chemical experiments and LAVOISIER's sound scientific logic led him to renounce the phlogiston theory in the following vigorously worded statement in 1783: "Chemists have made a vague principle of phlogiston which is not strictly defined, and which in consequence accommodates itself to every explanation into which it is pressed. Sometimes this principle is heavy and sometimes it is not; sometimes it is free fire, and sometimes it is fire combined with the earthy element; sometimes it passes through the pores of vessels, and sometimes they are impenetrable to it; it explains at once causticity and non-causticity, transparency and opacity, colors and the absence of colors. It is a veritable Proteus which changes its form at every moment."

Some years later LAVOISIER stated his reasons for discarding *in toto* the theory of phlogiston in favor of the new chemical science. He graciously admitted that the phlogiston theory had been like scaffolding which enabled later investigators to erect the edifice of chemical theory and that they were then at a point where it was possible to take down the scaffolding. He recognized that the fixed air which was obtained by the combustion of charcoal was composed of nothing but carbon and oxygen. He conducted analyses which led him to a very close approximation of the actual ratio of carbon and oxygen in carbon dioxide. Having arrived at this important discovery, LAVOISIER was then in a position to attack the problem of the real nature of organic substances. His genius enabled him to leap to the conclusion that since organic substances give off mainly fixed air and water when burned, they must be composed largely of carbon, hydrogen and oxygen. The fury of the French Revolution and LAVOISIER's martyrdom were not sufficient to extinguish the light which he had lit and which guided many a future investigator along the correct pathway for coming to an intelligent comprehension of the chemical nature of the world and the things therein.

We have, unfortunately, no definite statements concerning the influence of LAVOISIER's work on INGENHOUSZ, but it seems justifiable to conclude that they were personally acquainted and that INGENHOUSZ was pretty familiar with the conclusions which LAVOISIER had drawn from his careful analytical work. LAVOISIER's epoch-making work, Traité Élémentaire de la Chimie, was not published until 1789, but his ideas had been communicated previously in notes to the Academy and in letters to prominent men.

How well INGENHOUSZ comprehended all the discoveries of LAVOISIER is not recorded. We can feel confident, however, that he realized that the nature of pure and vitiated air was concerned with the presence or absence of oxygen.

JEAN SENEBIER, a Swiss natural philosopher, engaged in many scientific debates on photosynthesis with INGENHOUSZ and often disagreed with his conclusions. SENEBIER was the son of a wealthy merchant, well educated, and conversant with the scientific literature of his day (*cf.* BAY, 1931). His principal writings were on plant physiological, meteorological and chemical subjects.

SENEBIER's superior technique showed that more oxygen was evolved from submerged leaves if the water had been charged with carbon dioxide, but less oxygen if submersed acquatics were employed. He was convinced that the conversion of "fixed" into "pure" air occurs in the interior of the leaf.

SENEBIER was critical of some of INGENHOUSZ' earlier experiments, but substantiated his discovery that oxygen is evolved only by green plants in the sunlight and that none is evolved by etiolated leaves, flowers, and similar structures. He demonstrated, not only, that there must be access to carbon dioxide for photosynthesis to occur, but that the amount of oxygen liberated in the process bears a certain relation to the amount of carbon dioxide in the water. His opinions about respiration were nebulous. He never convinced himself that carbon dioxide is produced by healthy foliage, but believed it to be a product of pathological material. He expressed his ideas as follows: "It is here that one must distinguish scrupulously the air produced by the fermentation of leaves which are vitiated, from the air which they liberate when the sun urges them to exhale it; I doubt not that it is this lack of attention which has led to a calumny on Nature and plants in attributing to them the dangerous faculty of exhaling, during the night, gases capable of diminishing the purity of the atmospheric air by their noxious qualities."

The arguments between INGENHOUSZ and SENEBIER have relatively little interest for us today. It is difficult to decide the extent of the influence of one upon the other. SENEBIER's work may be less the fruit of INGENHOUSZ than of scientific ideas which he had previously communicated to BONNET, but there is no question that SENEBIER was influenced by INGENHOUSZ.

In the first pages of his work (Expériences sur l'action de la lumière solaire dans la végétation. Genève, 1788) SENEBIER attempted to refute some criticisms which INGENHOUSZ had published. There was evidently a question of priority of publication, but not merely that. "There are two things to distinguish in the remarks of M. INGENHOUSZ on my letter; the first includes the discussions on physics where I hoped to prove clearly and honestly that the opinions and remarks of M. INGENHOUSZ have not always the solidity that he wished us to believe; but that matter is too broad and too intricate to be treated now" (p. 27). "The second includes the inculpations, the expressions which my character prevents me to qualify, in ordering me to repel them with force. I restrain myself at present to oppose, as I have said, two facts well demonstrated to two ideas of M. INGENHOUSZ." In the first he proclaims his honesty in giving recognition to INGENHOUSZ and his work; in the second he tried to establish that his results of the effects of acidulated waters on leaves were obtained independently.

PORTRAITS OF INGENHOUSZ. — *At right:* photograph of an engraving by MATHIEU, Vienna, 1802, from a wax medallion which bore the legend 'KOLLONITZ f. 1770'. (*Courtesy* of Dr. J. C. BAY, John Crerar Library). — *Top, left:* bronze medallion made in England at the time of INGENHOUSZ' election as a member of the Royal Society of London (*cf.* VAN LOON, p. 484, no. 853 — the die is still in the possession of Mr. GÉRARD INGEN-HOUSZ of Breda.) (*Courtesy* Breda Municipal Archives). — *Bottom, left:* postage stamp (one of a series issued with a surtax for charitable purposes, Netherlands, Summer 1946), probably after the well known bust in Vienna.

ORNATISSIMO EXPE[illegible]MOQUE DOMINO
D O M I N O
JOANNI INGENHOUSZ
B R E [illegible] A N O
In Alma Uni[illegible]te Lovaniensi
MEDICINÆ [illegible]UREAM
SUMMA CU[illegible]SEQUENTI
DIE XXIV. JULII M.DCC.LIII.

Reda per omnigenas Urbs [illegible] Terras,
Inter [illegible] vix ullis celebrior Urbes,
Dives opum, formosa, potens & Martis & Artis,
Mœnibus ac Domibus [illegible];
Omnia, quæ magnis operum [illegible],
Excellens, & juncta [illegible]
Ad Zo[illegible]. Sed quis [illegible],
Templa, [illegible] Arces, & Tecta [illegible]
[illegible] Marmoreis? [illegible]
[illegible] visis, [illegible],
Ac longa [illegible]. Hic Principes Aula
Auriacæ, [illegible]
[illegible] Caput [illegible] Decorum.
Hic Ingenhousiana [illegible] ab Annis
[illegible]
[illegible]
[illegible]
Patria [illegible],
[illegible]
[illegible] Rector (cui magna Scholarum
[illegible]);
[illegible]
Tota [illegible] fervore Caterva;
Nec mirum: magni [illegible] *Tulli*,
Eloquiique lepos & abundans vena *Maronis*
Caput ob auspiciis concendit ad ardua solers
Mens Ingenhousii; vicit labor hunc dextro
Serta ferens, [illegible] Musis assuevit, & inter
Tot Socios [illegible] erat præcurrere cunctis
Scilicet ingenii [illegible] Vigor, insitaque [illegible]
Vis animo, [illegible] Senex prudentia in Annis
Prodiderat se se, seu prima Elementa vel Artes
Socraticas doctis [illegible] pulvere Victor,
Et vel Apollineâ [illegible] Famâ
Posset, & ad Patriæ majorem assurgere Laudem.
Ut quondam Proavi, & Patrui Praeclaria [illegible]
Pectora, Mavortis medias volitare per Ignes
Ausa, per adversas acies hostilesque Arma
Intrepidis ivêre animis, fortesque cadendo
Effudêre animas, quorum [illegible] Avita
Virtus, commilitosque sibi ad fera Bella trahebat
Bellonæ Pugiles; ceciderunt ante *Duarum*
Ambo [illegible]
Sponte sua Patrum, vastum dum terruit Orbem
Eugenius, cæsisque amplos tulit hostis triumphos
Gloria Belligerum, mundi Decus, *Austria* Victrix
Principe quo vidit Saturnia currere Secla
[illegible] Heroas inter præcellere cunctos
Inter [illegible] Duces. Illorum sunt sponte secuti
Ambo Ingenhousii *Batavo* cum Milite, & Ambos
Intrepidos Bellona Viros experta, fideles
In Patriam, contra quoscumque animosius hostes
Omnem cum vita penitus fudisse Cruorem.
Sic Pater à Patruis potuit non degener esse.
Ille *Hollandorum* quoque per duo Lustra secutus
Agmina, nec metuit Caput objectare periclis,
Nec redit è Bellis sine vulnere: vulnera testor,
Quasque cicatrices animosa in fronte notamus,
Martis signa cano, celebrat quæ Patria tellus
Quæque olim seri poterunt celebrare Nepotes.
Quis claros memorabit Avos? Proavûmque decora
Fasta recensebit? Celebres quos Marte Togâque
Et latè insignes meritis coluêre *Sicambri*,
Quorum immortalis vivit post funera Virtus,
Nescia quæ Lethi est, æternâ Laude perennans.
Tu magni Hippocratis fers, *Expertissime*, Lauros,
Et pro [illegible]
Castra sequi, miserisque ægris afferre Medelam,
Languentesque animos salvans salubribus herbis,
Hinc illustre Caput prudens *Medus* decorat;
Hæc tibi jam Medicâ nectit de fronde Coronam,
Ut vigiles Patriæ pro prosperitate labores
Susceptos, Proavumque quem pro more mereri
De Publico, Nomenque tuum vulgare per Orbem
Tu quondam doctæ fulsisti stella juventæ,
Gnaviter à Puero junxisti Græca Latinis
Omnibus excellens studiis, e pectore prompto
Gallica verba fluunt, dedit hoc Idioma *Duacum*,
Dum Ciceronis opes hausisti, & Nectara Pindi
Imbiberas avidus, numquam exsatiatus ab illis
Discere plura cupis, rerum dum plurima calles.
Te linguæ [illegible] honos, te copia fandi
Et Doctrina beat Græcis ex fontibus hausta.
Jamque ubi dignum *Grudiana* Academia Sertum
Contulit, Expertus nôsti, quæ Pharmaca quondam
Miscuerat *Chiron*, sanosque reduxit ad usus,
Sic ab Apollinea rediisti Victor arena
Promeritis ornatus Lauro Palmaque decenti
Conspicuus, Nostri decor & nova Gloria Cœtus
Et nova Spes *Bredæ* pergas depellere morbos
Finibus à Patriis, Pyliique attinge senectam
Incolumis, sic Corda Tibi *Bredana* precantur

CHRONO-DISTICHON

VICIstI eXpertUs, præCeLLIs apoLLInIs UsU:
eCCe eXornatUs DebIta Dona CapIs.

Applaudit Inclyta Congregatio Philosophorum [illegible]

LOVANII, typis JOANNIS JACOBS, plateâ [illegible] sub signo [illegible] Nigri.

BROADSIDE POEM, ISSUED IN LOUVAIN AT THE OCCASION OF INGENHOUSZ' DOCTOR'S PROMOTION (175

PLANT PHYSIOLOGICAL INVESTIGATIONS

Illustrious men of the eighteenth century broke the ground and laid solid foundations on which others have built the edifice of experimental biology. Our contemporaries are enlarging and sometimes remodeling parts of the structure, but we remember with gratitude the basic work of those able men who invented ways of making living plants answer questions about themselves. Most of them were not botanists in the modern sense of the word, but to their trenchant labors modern plant physiology owes its foundation, which, in connection with plant anatomy, made significant progress in the next century. INGENHOUSZ' physiological investigations were meritorious beyond question, but it would be difficult to put him into the same group with DE SAUSSURE.

The work of HALES had its development contemporaneously with the discoveries of BOYLE on the laws of physics and likewise the work of INGENHOUSZ was accompanied by the rise and spread of the knowledge of chemistry in the latter part of the eighteenth century.

INGENHOUSZ' five elemental statements concerning the phenomena of nutrition and gas exchange by plants, which have been given on a preceding page, show that he sketched the outlines of the processes of photosynthesis and respiration. He demonstrated the difference in the nature of the gases evolved by each process. He approximated, but did not demonstrate, that in plants the process of photosynthesis results in the production of energy-rich compounds and that in animals, the plant foods are broken down to release energy.

The merit in INGENHOUSZ' work seems to have been the ability to draw essentially proper conclusions from his data. Nowhere is it more clearly shown than in his reflections on the action of sunlight on plants. He correctly concluded that it is only the green plant organs which release oxygen in sunlight. Non-green organs (flowers, fruit, roots) are positively unable to evolve oxygen. He stated, and this also was pre-eminently correct, that it is the underside of the leaf which especially releases the dephlogisticated air, a fact which fully agrees with our knowledge of the anatomical structure of the leaf blade. He understood another important thing, namely, that fully expanded leaves also excrete oxygen abundantly. So then, PRIESTLEY's assumption that the growth of the plant is the cause of the excretion of oxygen was again disproven. Much remains to be done to elucidate that subject, but at least he saw some of the important features of the process of photosynthesis.

As already intimated, the discovery of the relation of light to the evolution of oxygen by green plants was INGENHOUSZ' *magnum opus,* the thing for which he won fame in the annals of science. We cannot be certain how much insight into this problem of plant nutrition INGENHOUSZ had at the beginning of his investigation. His predecessors who had attacked the problem had confused the issue by introducing ideas from realms of science which had nothing to do with plants. All of them were still under the

influence of ARISTOTLE and his idea that the earth was a huge stomach in which food was elaborated for the plant and that the roots merely absorbed what had been digested in the earth. VAN HELMONT, MALPIGHI, MARIOTTE and HALES expressed views and set up experiments whose results showed that the Aristotelian doctrine could not possibly be correct. The scientific world, however, was not yet ready to adopt such views, because they seemed to set at naught the respectable conclusions of the classical writers of antiquity. The mind of man seldom frees itself from the shackles of the past at one stroke. Perhaps the struggle was nowhere better represented than in the case of DUHAMEL, who made several unsuccessful attempts to free himself from the Aristotelian doctrine. The results of his well-known water-culture experiment clearly contradicted ARISTOTLE's fundamental premise, and from them he was forced to the conclusion that water constituted an important part of the nutriment of the plant. That the solid parts of the plant originate in water, he doubted not from the results of his water-culture, but he remarked in this connection that he was unable to explain how this fluid state was transformed into the solid substance of the plant.

The interest of modern plant physiologists will be directed to the apparatus (p. 322, figs. 2 and 3) and the technique by which INGENHOUSZ succeeded in obtaining large amounts of oxygen from green plants. We note for this purpose that he placed the green leaves or green organs into a glass vessel filled with fresh spring water. "This vessel was then placed with the mouth downward in a bath filled with the same kind of water and put into full daylight, or better, in direct sunlight. The leaves continued to live and function sufficiently for the analysis of the gas, and the water entirely prevents the atmospheric air from being absorbed by the leaves but does not prevent the elimination of the air produced by them. The air which is evaporated in the leaves appears soon on the surface, mostly in the form of round bubbles which gradually increase in size, free themselves from the leaves, and rise into the bottom of the inverted receptacle. The air collected by this device is actually dephlogisticated air. It is not unusual to see air bubbles follow each other so rapidly that they arise in a continuous stream from one and the same place" (Experiments, p. 14-16; German translation, p. 35-36).

INGENHOUSZ said (p. 20), "These two examples show the manifold way in which the wholesome and pure air comes out of the leaves. One must ascribe these variations simply and solely to the circumstance that the organization of the leaf differs among different plants."

The last statement refuted at one stroke PRIESTLEY's erroneous views about the ability of water itself to purify the air. This is one of the amusing instances where PRIESTLEY was led to make erroneous conclusions from good observations. He discovered that when one fills a clear glass jug with water from a well or from a spring and inverts the same over water, it produces a green substance which is attached to the sides of the bottle and to the bottom of the bath on which the jug is placed. He saw that this green substance liberated a great number of bubbles of air which collected at the upper end of the inverted jug, and he found that it was dephlogisticated air, in which the flame of a candle burned more brightly than in ordinary air. Since this air is produced in the water, without any addition, he

concluded from the observation, and with apparent reason, that the large bodies of water such as oceans, lakes and rivers ought to contribute much to the purification of our atmosphere. It is rather gratuitous to point out that PRIESTLEY would have seen the error of his conclusions if he had used a magnifying glass for the examination of the green substance which developed on the inner walls of the glass jug.

INGENHOUSZ himself was not altogether successful in his dealings with the problem which PRIESTLEY described. INGENHOUSZ said in his book, (French edition, section 5), "I do not know whether one should suppose justly that the dephlogisticated air thus obtained from the water after this green substance had been developed there is a gas inherent in the water. However that may be, the case is not applicable to that of the leaves of plants plunged beneath the water, for in the case of the green substance, it requires some days before it produces a perceptible amount of dephlogisticated air, which indicates that it is not the water but the green matter which produces this air." INGENHOUSZ logically said, "That if this wonderful power of the plants to produce dephlogisticated air was related to their power of growth, it would express itself at all times and in all places where only a plant can occur. A plant can exist and grow in the darkest places where they do not, however, have the power of improving the common air or even of producing pure air. On the contrary, they even spread harmful vapors which have the power of poisoning the purest air. Moreover, it would not be difficult to examine the causes which gave rise to the variable results of Dr. PRIESTLEY's experiments even to some contradiction."

The bubbles of air on the surface of leaves under water presented a problem which INGENHOUSZ attacked with his characteristic ability to see all sides of the problem. He pointed out that natural water contains a quantity of air which one might expel by means of heat. The waters of springs, especially, contain a considerable portion of air, and some of them such an abundance that they may break bottles in which one has placed corks after completely filling the bottles. If such waters contain a sufficient quantity of air to give them a decided acidulous taste, they are classed among mineral waters. This gas in the water is commonly known as fixed air, or the native gas, according to VAN HELMONT. This air, which is in truth acid, gives to those waters the property of dissolving iron. They may be changed by a rather simple and well-known means into ferruginous waters by adding to them some nails or other pieces of iron.

The water which is the most agreeable for drinking purposes owes its taste principally to the air which it contains. Distilled water is tasteless because it has lost its air.

INGENHOUSZ examined pump water which contained a rather large quantity of air. The most simple manner of obtaining this air without alteration was to expose the water to the sun in glass jugs, filled and inverted. Having thus exposed fifteen or sixteen jugs to the sun, he obtained in a few hours a quantity of air sufficient for analysis. He found that the air thus collected was inferior in quality to the atmospheric air. The water from certain springs and perhaps the water from all rivers contains an air much more pure than the common air. The water of the Danube and of the Seine contains a rather large quantity of it. This observation confirmed him in the idea, rather generally received, that the neighbor-

hood of a river which does not leave its bed to make marshes renders the air healthy.

INGENHOUSZ filled a cylindrical jar with this same water from the well and inverted it with care in a clean jar. This apparatus was put on the fire until the entire quantity of water, even that which filled the inverted, cylindrical jar, was in full ebullition. The air released from the water by this means rose to the bottom of the cylindrical, inverted jar. When everything was cool, he examined this air and found it much worse than that which was liberated from the water spontaneously in the sunlight. In fact, it was so vitiated that it caused pain of the animal which inhaled it.

In section 6, INGENHOUSZ directs his readers to a consideration of experiments to prove that the dephlogisticated air which escapes from the leaves under water does not exist in the leaves as such but that it comes from the leaves after having undergone a process of "purification" or a sort of "transmutation." In this section we see little evidence that INGENHOUSZ was familiar with the fundamental chemical ideas of LAVOISIER. He says that he has successfully demonstrated that the dephlogisticated air obtained from the leaves comes out of the leaves themselves, and ventured to express the belief that it ought to exist as such in the substance of the leaves and that it would be possible to extract it by the heat of the fire.

We often think of INGENHOUSZ in connection with his great discovery concerning the utilization of gaseous carbon dioxide by plant, but he studied more widely the gasic changes in plants. He demonstrated the great oxygen requirement of germinating seeds and their elimination of carbon dioxide even in the absence of oxygen. His penetrating mind was engaged in consideration of the influence of light on germination of seeds, and he showed that light hastens the germination of most seeds but that an electrical current had no effect upon germination.

INGENHOUSZ also studied the influence of light intensity on plant activities. Thus, on the process of germination and on that of oxygen elimination by green organs, he showed that in many plants it was not the greatest light intensity which cause the greatest evolution of oxygen. On the contrary, even in very weak diffused light, oxygen evolution proceeded at a rate which was indicative of proper physiological balance.

His observations on various biological problems were published in treatises entitled "On the Economy of Plants." These papers dealt with questions such as the distinction between plant and animal life, the irritability of many plants like *Mimosa* and *Hedysarum,* and the so-called sleep of plants. Some of the experiments showed that the fragrant odors emitted at night by many plants, such as *Hesperis tristis* and *Geranium triste,* are not simply the result of the absence of light but must have a deeper basis in the organism since plants put into darkness during the day under otherwise normal light conditions do not produce the aforementioned odors as during the night.

In view of the discoveries of FRANKLIN in the field of electricity, it was natural that INGENHOUSZ should investigate its effect upon living plants. He devoted no little attention to the effect of atmospheric electricity on the growth of plants, but had the honesty to admit that there was no perceptible effect. THOMAS JEFFERSON paid some attention to these phases of INGENHOUSZ' work, but seemed to have been unconvinced of the

verity of the new ideas on photosynthesis. His attitude may be judged from a letter he wrote from Paris, July 19, 1788, to President MADISON of William and Mary College. (I quote from BROWNE, Chronica Bot. 8, p. 380, 1944).

"You know also that Dr. INGENHOUSZ had discovered, as he supposed, from experiment that vegetation might be promoted by occasioning streams of the electrical fluid to pass through a plant, and that other physicians had received and confirmed this theory. He now, however, retracts it, and finds by more decisive experiments, that the electrical fluid can neither forward or retard vegetation. Uncorrected still of the rage of drawing general conclusions from partial and equivocal observations, he hazards the opinion that light promotes vegetation. I have heretofore supposed that light affects the color of living bodies, whether vegetable or animal; but that either the one or the other receives nutriment from that fluid, must be permitted to be doubted of, till better confirmed by observation. It is always better to have no ideas than false ones; to believe nothing, than to believe what is wrong. In my mind, theories are more easily demolished than rebuilt."

The phenomenon of plant nutrition always baffled INGENHOUSZ although he eventually accepted many of SENEBIER's ideas on the subject. His confusion was displayed in his "Essay on the Food of Plants and the Renovation of Soils" (1796) wherein he expressed a belief that carbon dioxide is absorbed by plants, though uncertain whether all is absorbed by leaves or partly by roots. He made some pertinent observations on the liberation of carbon dioxide by soils. Having learned that organic manures incorporated with the soil disengage a great quantity of carbon dioxide, it seemed probable to him that the principal attention in manuring ought to be directed to the time of applying manure to the soil. If it is applied when at the height of its putrid fermentation, the growing plants may absorb the "fixed air," or carbon dioxide, either in a nascent state, or as it is being incorporated with various salts and earths which are to be found in almost all soils.

He could not know that microorganisms were responsible for most of this oxidation. Yet, he saw that siliceous sand, either dry or moist, does little or nothing at all to injure the air over it; nor is it materially affected by being shut up with pure water. He suggested that farmers add acids to the soil, since his experiments with them produced increases in plant growth.

The confusion of ideas expressed in these later studies was soon removed by the clarifying work of THÉODORE DE SAUSSURE (1804), whose simple experiments yielded quantitative results which contributed greatly to the understanding of processes in the living plant. The roles of carbon dioxide and of mineral constituents in plant nutrition were adequately and correctly described. The difference between respiration and photosynthesis was so succinctly shown that there has never again been confusion on that subject.

If one wishes to appraise INGENHOUSZ' contribution to science, let him consider the status of plant physiology before and after his time. It ought to be evident that he valiantly helped to break the shackles of the past, by conducting experiments on plants and by analyzing the results. Alive to the importance of contemporaneous advances in chemistry, he hoped that his

discovery might open the way to further progress. Starting with ideas on the effects of plants on the salubrity of the atmosphere, he arrived eventually in broader fields. His work stands the test of time, *viz.*, it stimulated others to work and to overthrow false conceptions of the nature of living plants.

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HISTORIOGRAPHIC NOTES

Few writers of his time excelled INGENHOUSZ in making their scientific work available to the public. The world owes a great debt to a scientific man who will take his pen and write about his discoveries instead of indulging his ambition to make a few more experiments. INGENHOUSZ knew that he had made a great discovery and recognized the obligation of putting it on record, furthermore, he defended his conclusions when they were attacked by others. He was not always right, but he was honest.

A brief statement about the time and manner of publishing his work on the gas exchange in plants and the effect of light thereon will be introduced as a means of orienting the reader.

INGENHOUSZ said almost nothing about the circumstances of publishing the English edition of 1779. The task of getting a reliable printer might have been facilitated by the fact that he was then an eminent court official (*see* title page) and enjoyed corresponding perquisites. To judge from the present scarcity of copies it was not a large edition, it was out of print early in 1780 and was not reprinted.

The first French edition (1780) was virtually a translation of the English by the author in fulfillment of the advertisement in the English edition. Subsequently, he wrote the French edition anew and issued it in two volumes. The first of them is said to have been completed in 1780 while he was in Paris, and the second in 1781 in Vienna. They did not, however, appear until 1787 and 1789, the delay being due, according to the author, to the faithlessness of the printer. INGENHOUSZ affirmed that he availed himself of his rights as an author to make such changes and additions as he felt would improve the work. As a result we have a much better book.

INGENHOUSZ was evidently so well satisfied with the reception of his ideas and confident that his discoveries could greatly contribute to human welfare that he wrote with more assurance in the new edition. Through discussions and correspondence with scientific leaders of the day in the interval between the editions, there is no doubt that his ideas on the evolution of oxygen by plants had been well rounded and polished. He gave the details of his technique and took the opportunity to point out the reasons for the failures of some experimenters who had obtained discordant results. He disavowed any envy of others who had written on the subject, yet, with some impeccability, affirmed that his ideas had been endorsed by all right-thinking scientists.

INGENHOUSZ followed the plan of the English edition whereby he separated the expository material from the protocol of the experiments. The first was entitled "Observations sur la nature des plantes," the second, "Contenant une suite d'expériences faites avec des feuilles, des fleurs, des fruits, des tiges and des racines des plantes, dans le dessein d'examiner la nature de l'air qui s' évapore de ces substances, et de montrer leur influence sur l'air commun dans différentes circonstances."

The second French edition contained new material, the outcome of researches INGENHOUSZ made in Vienna in 1780 and subsequently. It displayed a prolixity which is in contrast with his terse English edition, but is more philosophical in style.

We have inserted in appendix I, the first section of the French edition in order that readers who possess a modicum of interest in the progress of scientific exposition may critically compare the two texts.

Plant physiological ideas emerge more clearly in the French edition, references being made to the works of NEHEMIAH GREW, HENRI DUHAMEL DU MONCEAU, CHARLES BONNET, and others. INGENHOUSZ still wavered in his acceptance of the discoveries of LAVOISIER on the composition of water, namely, that it is composed of inflammable air (hydrogen) combined with dephlogisticated air (oxygen). He felt that the evidence was not sufficiently conclusive. In a footnote on page 192 he remarked, nevertheless, that the new system of chemistry formulated by LAVOISIER would make a great revolution, but that it had not then been adopted by more than a few chemists.

The English edition was translated into Dutch by J. VAN BREDA and published at Delft in 1780. INGENHOUSZ contributed new material and said (avant propos. Fr. ed. p. X) that the translation was more complete than his own edition.

The first German edition appeared also in 1780. The translator is not known, but INGENHOUSZ commended his work. A second German edition in three volumes was published at Vienna in 1786-90. It was said to be a translation of INGENHOUSZ' French edition. This translation was made by a Viennese physician, Dr. J. A. SCHERER, with whom INGENHOUSZ was well acquainted and had often discussed the subjects treated in the book.

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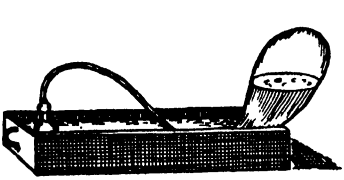
INGENHOUSZ' EXPERIMENTS

upon

VEGETABLES

(London, 1799)



EXPERIMENTS

UPON

VEGETABLES,

DISCOVERING

Their great Power of purifying the Common Air in the Sun-ſhine,

AND OF

Injuring it in the Shade and at Night.

TO WHICH IS JOINED,

A new Method of examining the accurate Degree of Salubrity of the Atmoſphere.

By JOHN INGEN-HOUSZ,
Counſellor of the Court and Body Phyſician to their IMPERIAL and ROYAL MAJESTIES,
F. R. S. &c. &c.

LONDON:
Printed for P. ELMSLY, in the Strand; and H. PAYNE, in Pall Mall. 1779.

TITLE PAGE OF INGENHOUSZ' FIRST BOOK ON HIS 'EXPERIMENTS' ISSUED IN ENGLISH AND PUBLISHED IN LONDON IN 1779.

To Sir JOHN PRINGLE, Bart.

Physician to his Britannic Majesty, late President of the Royal Society, Member of the Royal Academy of Paris, etc. etc.

Sir,

A grateful remembrance of past services is as just a tribute due to those from whom they are received as the acquitting of a debt contracted in any other manner. If it is not in the power of a man to make a return suitable to the benefits received, he is, however, in duty bound to show, by the best method in his power, a thankful heart to his benefactor.

Ingratitude was by the ancient Greeks held as a crime of the blackest dye, as tending directly to destroy the motives of mutual benevolence, and to dissolve the ties of friendship, that source of human happiness, without which life itself is scarcely worth enjoying.

The ungrateful, conscious of his misbehaviour, and looking upon his benefactor as upon a judge who has pronounced a just and severe sentence upon him, endeavours to find reason for breaking off with him; while his benefactor, looking upon the ungrateful as upon a monster unworthy of his regard, is induced to shut, for the future, his heart against others.

No man upon earth can have stronger reasons for a due sense of gratitude than I acknowledge to you. You bestowed many civilities upon me, who had never been in the way of doing you any service whatever. You granted me your friendship almost as soon as I was acquainted with you. You encouraged my eagerness for improving myself in medical knowledge, by communicating to me what you had learned by a laborious life; by that experience which an assiduous and most attentive zeal, bestowed in the care of the great military hospitals in the time of war, and a most successful private practice, had afforded you, and of which your celebrated work upon *The Diseases of the Army* will be an honourable and everlasting testimony, as well as a real benefit, to the latest posterity.

You always gave me, with the greatest sincerity, your advice in what manner, and in whose company, I could most improve myself in the various branches of medicine and natural knowledge which I took a delight to cultivate.

It was you, sir, who, among those many respectable and learned men, whose constant friendship towards me has made an indelible and grateful impression on my mind, contributed principally to that particular happiness I enjoyed during so many years in this island; that felicity which a free and independent man finds in the pursuit of knowledge and wisdom in the society and friendly intercourse of those who have distinguished themselves by their learning.

But, sir, among the many obligations which I owe you, there is one of such importance, that the very thought of it strikes me with reverence and with the deepest sense of gratitude for you. You did me a service which I cannot forbear to mention; though I know that your modesty would hardly permit me to express the true situation of my mind in that respect.

Permit me, sir, to leave behind me some public testimony of my respectful gratitude to you, as the only, though small, return I can make you; the only way by which I can publicly shew, that the unsolicited favours so generously bestowed upon a foreigner, who could not claim the least merit with you, have made so strong an impression on my mind as no time is able to weaken. You have recommended me, sir, without my soliciting any favour from you, to those August Sovereigns who are still the support of the illustrious House of Austria; those powerful Monarchs whose graciousness, benevolence, and magnanimity, equal the supreme grandeur of their station. These August Sovereigns, after having suffered so many repeated losses by that dreadful disease the Small-pox, resolved at last to check that terrible havock in their illustrious Family, and ordered their Ambassador to send to their Court a physician from this island, capable of fulfilling the important trust of saving, by means of inoculation, the remainder of the Royal Offspring, which had as yet escaped the infection. Being consulted on the choice of a proper person, you proposed me without hesitation, and thus opened to me a wide door to emoluments and honours.

After having been so publicly and so honourably called from a distant country to the most generous and powerful Monarchs; and after having contributed to the tranquillity and happiness of so many illustrious Princes, who, being educated under the maternal care of the most virtuous Princess, are become highly important to mankind, and have filled the world with a well-founded confidence to see its happiness promoted by their means; whatever advantage or reputation I have acquired from such a flattering appointment, I derive it all from your friendship.

My earnest desire of not quitting this country without leaving you some public testimony of my real sentiments towards you, excited me to hurry this work to the press without having time enough to finish it as I desired. If it had been in my power to have spent the ensuing winter in this country, I might possibly have made it more worthy your patronage, and of appearing in the world under your auspices. I present it to you imperfect as it is; and beg of you to look upon it only as a public mark of my respect and gratitude, which I shall retain in full force to the end of my life, and with which I have the honour of subscribing myself,

Sir,
Your very much obliged
and faithful friend and servant,
J. Ingen-Housz

London, October 12, 1779

PREFACE

The common air, that element in which we live, that invisible fluid which surrounds the whole earth, has never been so much the object of contemplation as it has in our days: it never engaged so much the attention of the learned as it has of late years. This fluid, diffused every where, *the breath of life,* deserves so much the more the attention and investigation of philosophers, as it is the only substance without which we can scarce subsist alive a single moment, and whose good or bad qualities have the greatest influence upon our constitution. The most active poisons which are known do not so quickly destroy the life of an animal as the want of air, or the breathing of it when it is rendered highly noxious. It will appear in this work, that those very plants, which, influenced by the light of the sun, repair the injury done to this fluid by the breathing of animals, and by many other causes, may, in different circumstances, poison so much this very element, as to render it absolutely unfit for respiration, and, instead of keeping up life, to extinguish it in a moment. Therefore this universally-diffused element deserves not only the pursuit of philosophers, but claims more immediately the attention of those whose profession it is to preserve health and to cure diseases. I have bestowed some labour upon this subject, both as a philosopher and as a physician.

When I first found in the works of that excellent philosopher and inventive genius, the reverend Dr. PRIESTLEY, his important discovery, that plants wonderfully thrive in putrid air; and that the vegetation of a plant could correct air fouled by the burning of a candle, and restore it again to its former purity and fitness for supporting flame, and for the respiration of animals; I was struck with admiration: and I could not read afterwards, but with a kind of extasy, the application which Sir JOHN PRINGLE made of this discovery in his elaborate discourse, delivered at the Royal Society in November 1773, when he conferred, as president of that learned Body, the annual prize medal upon Dr. PRIESTLEY, decreed to him as an honourable testimony of their approbation of the successful labours bestowed by him upon the doctrine of air. "From these discoveries," says he, "we are assured, that no vegetable grows in vain, but that, from the oak of the forest to the grass in the field, every individual plant is serviceable to mankind; if not always distinguished by some private virtue, yet making a part of the whole, which cleanses and purifies our atmosphere. In this the fragrant rose and deadly night-shade cooperate: nor is the herbage, nor the woods that flourish in the most remote and unpeopled regions, unprofitable to us, nor we to them; considering how constantly the winds convey to them our vitiated air for our relief, and for their nourishment. And if ever these salutary gales rise to storms and hurricanes, let us still trace and revere the ways of a beneficent Being, who not fortuitously, but with design, not in wrath, but in mercy, thus shakes the waters and the air together, to bury in the deep those putrid and pestilential effluvia which the vegetables upon the face of the earth had been insufficient to consume."

Since I read that elegant discourse, I have wished that some industrious philosopher would bestow his labour in tracing Nature in its operation, and in discovering the manner in which the vegetable kingdom is subservient to the animal, in correcting the mass of air contaminated by their respiration, and perhaps too by their perspiration. The following pages will show, whether the pains I took, in the course of this summer, to investigate this important subject, have been attended with some degree of success. I am far from thinking that I have discovered the whole of this salutary operation of the vegetable kingdom; but I cannot but flatter myself, that I have at least proceeded a step farther than others, and opened a new path for penetrating deeper into this mysterious labyrinth.

Among the various useful discoveries with which Dr. PRIESTLEY has already enriched, and still continues to enrich, natural knowledge, none, in my opinion, are of more importance than those he made upon the various kinds of airs.

The discovery of that wonderful aerial fluid, which in purity and fitness for respiration so far exceeds the best atmospheric air, that an animal may protract its life in it five times longer than in the best common air, excites so much the greater ad-

miration, as he found it first in such bodies which by their nature must have been suspected to conceal rather within their substance deleterious qualities, such as calcined mercury and red precipitate. He has given to this air the very proper appellation of *dephlogisticated* air, or air deprived of that inflammable principle which is the chief ingredient that renders our atmospheric air more or less impure, and thus more or less fit for respiration.

His discovery of that peculiar quality which nitrous air possesses, of destroying or being destroyed by common air in proportion to its purity, is one of those inventions whose utility will be more and more conspicuous, when it shall have undergone all the improvements of which it is susceptible. Let it be mentioned to his honour, that his candour and modesty have made him under-rate the value of this useful production of his inquiries, when he says, in his last work, intitled, *Experiments and Observations relating to various Branches of Natural Philosophy, with a Continuation of the Observations of Air,* p. 269, "When I first discovered the property of nitrous air as a test of the wholesomeness of common air, I flattered myself that it might be of considerable practical use; and, particularly, that the air of distant places and countries might be brought and examined together with great ease and satisfaction: but I own, that hitherto I have rather been disappointed in my expectation from it." And he concludes thus: "I have frequently taken the open air in the most exposed places in this country, *at different times of the year,* and in different states of the *weather,* &c.; but never found the difference so great as the inaccuracy, arising from the method of making the trial, might easily amount to or exceed."

Since I saw the manner of putting different airs to the nitrous test, which Abbé FONTANA now makes use of, and which I have in my inquiries for the most part imitated, I cannot but think more favourable of the importance of this discovery than the author himself does. I even think with the Abbé, that, by using convenient and accurate instruments, and by observing to the greatest nicety all the manoeuvres of the operation constantly in the same way, we may with as much precision judge of the degree of purity of common air, as we now are able to judge of its degree of heat and cold by a good thermometer.

Indeed, by this method, even all the changes which the constitution of the atmosphere undergoes daily, in the same place, are observed with so much accuracy, that, by making ten observations with the same air, the difference will scarcely amount to 1/500dth of the two airs employed in the experiment.

The discovery of Dr. PRIESTLEY, that plants thrive better in foul air than in common and in dephlogisticated air, and that plants have a power of correcting bad air, has thrown a new and important light upon the arrangement of this world. It shews, even to a demonstration, that the vegetable kingdom is subservient to the animal; and, *vice versa,* that the air, spoiled and rendered noxious to animals by their breathing in it, serves to plants as a kind of nourishment. But in what manner this faculty of the plants is excited remained still unknown.

There was even some doubt left in the mind of many philosophers, whether the facts related by Dr. PRIESTLEY were not owing to some particular accident, as they had by no means been uniform; nay, had even been often contradictory, as he himself candidly owns (*see* vol. I, p. 91, &c. of Dr. PRIESTLEY's work on the subject of air, and his last work, p. 296); and as Mr. SCHEELE had constantly observed a contrary effect from beans.

Dr. PRIESTLEY acknowledges, p. 299, that, by repeating (1778) again his experiments, *they proved to be unfavourable to his former hypothesis.* "For," says he, "whether I made the experiments with air injured by respiration, the burning of candles, or any other phlogistic process, it did not grow better but worse; and the longer the plants continued in the air, the more phlogisticated it was." He proceeds thus farther; "I have had several instances of the air being undoubtedly meliorated by this process, especially by the shoots of strawberries, and some other plants, which I could, by bending, introduce into the jars or phials of air supported near them in the garden, when the roots continued in the earth.—I had other instances, no less unquestionable, of common air not only receiving no injury, but even considerable advantage, from the process, having been rendered in some measure dephlogisticated by it, so as to be much more diminished by nitrous air than before, a thing which I was far from expecting.—In most of the cases in which the plants failed to meliorate the air, they were either manifestly sickly, or at least did not grow and thrive, as they did most remarkably in my first experiments at Leeds, the reason of which I cannot dis-

cover.—In those instances in which the plants grew the best, they were, however, but sickly, as appeared by the leaves soon turning yellow, and falling off when the least motion was given to them. In some cases, however, as in those mentioned in vol. I, p. 91, I saw no particular reason why the air should not have been meliorated."

"Upon the whole, I still think it *probable*, that the vegetation of healthy plants, growing in situations natural to them, has a salutary effect on the air in which they grow. For one clear instance of the melioration of air in these circumstances should weigh against a hundred cases, in which the air is made worse by it."

Soon after, p. 305, he relates several instances in which a plant had, in the space of seven, eight, ten, or more days, effectually mended the foul air in which it was made to grow. P. 309, he relates a fact, in which a sprig of *winter savory*, kept growing in a jar from the 16th of June to the 20th, had improved the air evidently, which improvement he found by three repeated trials to be in the proportion of 1.275 to 1.375. He relates another instance, in which air was so much improved by a sprig of parsley growing in it, from the 16th of June to the 1st of July, that one measure of it with one of nitrous air occupied only one measure. After all, he concludes with the following words, p. 310: "When these observations are well considered, I think it will hardly be doubted but that there is something in the process of vegetation, or at least something usually *attending* it, that tends to meliorate the air, in which it is carried on, whatever be the *proximate cause* of this effect, whether it be the plants imbibing the phlogistic matter, as part of their nourishment, or whether the phlogiston unites with the vapour that is continually exhaled from them; though of the two opinions I should incline to the former."

Mr. SCHEELE is so far from thinking that air is meliorated by plants, that he even maintains, that vegetation has the same effect on air that respiration has. He allows, however, that plants do not grow so well in dephlogisticated as in common air.

At the end of Section XXXIII, in which he treats *of the spontaneous emission of dephlogisticated air from water in certain circumstances*, he speaks thus: "It will probably be imagined, that the result of the experiments recited in this Section throws some uncertainty on the result of those recited in this volume, from which I have concluded, that air is meliorated by the vegetation of plants, especially as the water, by which they were confined, was exposed to the open air, and the sun, in a garden. To this I can only say, that I was not then aware of the effect of these circumstances, and that I have represented the naked *facts* as I observed them; and, having no great attachment to any particular *hypothesis*, I am very willing that my reader should draw his own conclusions for himself." Dr. PRIESTLEY, having observed that bubbles of air seemed to issue spontaneously from the stalks and roots of several plants kept in the water, suspected immediately, that perhaps this air, if found better than common air, had been percolated through the plant, and purified by leaving its phlogiston in the plant as its nourishment. With this view he plunged many phials containing sprigs of mint in water, laying them in such a manner, as that any air, which might be discharged from the roots, would be retained in the phials, the bottoms being a little elevated. In this position the springs of mint grew very well, and in some of the phials he observed a quantity of air to be collected, though very slowly; but he was much disappointed, that some of the most vigorous plants produced no air at all. At length, however, from about ten plants he collected, in the course of a week, about an ounce-measure of air, which he found so pure, that one measure of it and one of nitrous air occupied the space of only one measure.

This remarkable fact contributed not a little to confirm his faith in the hypothesis of the purification of the atmosphere by vegetation; but he did not enjoy this satisfaction long; for, as he found that other plants of the same species produced no such effect, and that what he thought more extraordinary, the phials, in which the above mentioned plants had grown, the inside of which were covered with a green kind of matter; continued to yield air as well when the plants were out of them as they had done before; he was convinced, that the plants had not, as he had imagined, contributed any thing to the production of this pure air. *See* Dr. PRIESTLEY's last work, p. 337 and 338.

Thus far this matter was carried on when I took it up in June last. I must acknowledge, that, from what is above related from Dr. PRIESTLEY's works, I had little doubt but there was some quality in plants proper for correcting bad air, and improving ordinary air. My curiosity led me to investigate in what manner this operation is carried on, whether the plants mend air by absorbing, as part of their nourishment,

the phlogistic matter, and leaving thus the remainder of the air pure (to which opinion Dr. PRIESTLEY inclines the most); or whether perhaps the plants possess some particular virtue hitherto unknown, by which they change bad air into good air, and good into better, which I suspected to be the case.

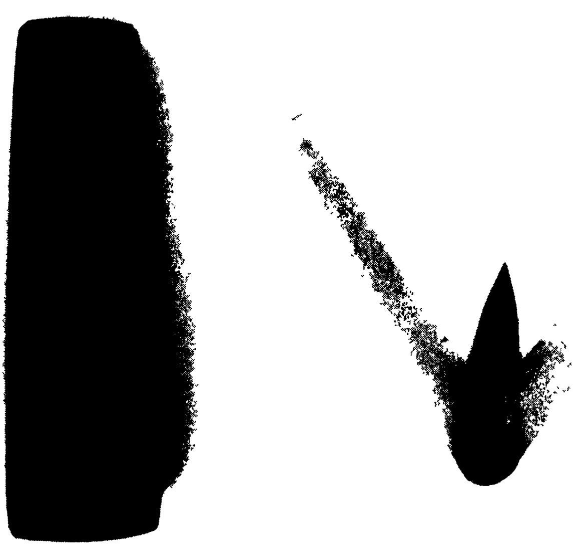
I was not long engaged in this enquiry before I saw a most important scene opened to my view; I observed, *that plants not only have a faculty to correct bad air in six or ten days, by growing in it, as the experiments of Dr. Priestley indicate, but that they perform this important office in a compleat manner in a few hours; that this wonderful operation is by no means owing to the vegetation of the plant, but to the influence of the light of the sun upon the plant. I found that plants have, moreover, a most surprizing faculty of elaborating the air which they contain, and undoubtedly absorb continually from the common atmosphere, into real and fine dephlogisticated air; that they pour down continually, if I may so express myself, a shower of this depurated air, which, diffusing itself through the common mass of the atmosphere contributes to render it more fit for animal life; that this operation is far from being carried on constantly, but begins only after the sun has for some time made his appearance above the horizon, and has, by his influence, prepared the plants to begin anew their beneficial operation upon the air, and thus upon the animal creation, which was stopt during the darkness of the night; that this operation of the plants is more or less brisk in proportion to the clearness of the day, and the exposition of the plants more or less adapted to receive the direct influence of that great luminary; that plants shaded by high buildings, or growing under a dark shade of other plants, do not perform this office, but, on the contrary, throw out an air hurtful to animals, and even contaminate the air which surrounds them; that this operation of plants diminishes towards the close of the day, and ceases entirely at sun-set, except in a few plants, which continue this duty somewhat longer than others; that this office is not performed by the whole plant, but only by the leaves and the green stalks that support them; that acrid, ill-scented, and even the most poisonous plants perform this office in common with the mildest and the most salutary; that the most part of leaves pour out the greatest quantity of this dephlogisticated air from their under surface, principally those of lofty trees; that young leaves, not yet come to their full perfection, yield dephlogisticated air less in quantity, and of an inferior quality, than what is produced by full-grown and old leaves; that some plants elaborate dephlogisticated air better than others; that some of the aquatic plants seem to excell in this operation; that all plants contaminate the surrounding air by night, and even in the day-time in shaded places; that, however, some of those which are inferior to none in yielding beneficial air in the sun-shine, surpass others in the power of infecting the circumambient air in the dark, even to such a degree, that in a few hours they render a great body of good air so noxious, that an animal placed in it losses its life in a few seconds; that all flowers render the surrounding air highly noxious, equally by night and by day; that the roots removed from the ground do the same, some few, however, excepted; but that in general fruits have the same deleterious quality at all times, though principally in the dark, and many to such an astonishing degree, that even some of those fruits which are the most delicious, as, for instance, peaches, contaminate so much the common air as would endanger us to lose our lives, if we were shut up in a room in which a great deal of such fruits are stored up; that the sun by itself has no power to mend air without the concurrence of plants, but on the contrary is apt to contaminate it.*

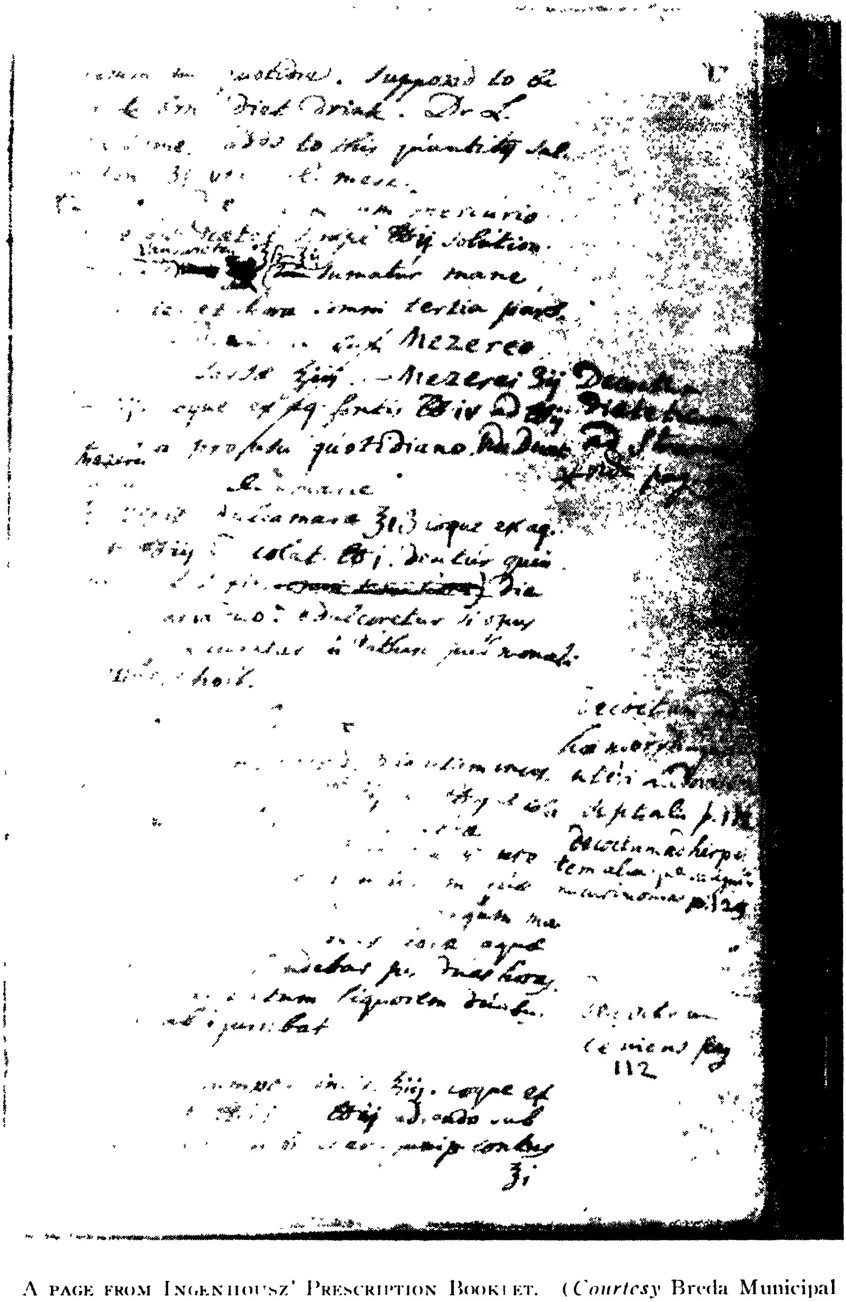
These are some of the secret operations of plants I discovered in my retirement, of which I will endeavour to give some account in the following pages; submitting, however, to the judgment of the candid reader the consequences, which I thought might fairly be deduced from the facts I am to relate.

I must not omit to acquaint the reader, that, in pursuing the experiments related in this work, he will find that he labours in vain, if he does not make use of pump-water freshly drawn; for if this water has been exposed to the open air during some time, it will have parted with a great deal of its own air, and will therefore be apt to absorb the air from the leaves. It may also happen, that every pump-water may not be found equally as good as that which I met with in my country dwelling, though as yet I have no positive reason to think so; but I have some grounds to believe, that water drawn from an open well is far inferior in goodness to that which is forced up by a pump, as the former is too much exposed to the open air.

By casting an eye upon the experiments related in this work, it will be easily understood, why, in every experiment of this kind, some difference in the result will

Above: Medal given to INGENHOUSZ by the Empress MARIA THERESIA after the successful inoculation of children of the Imperial Household (1768.) Lost by fire at Arnhem in 1944. — *Below:* Inoculation instruments used by INGENHOUSZ (*Courtesy* Breda Municipal Archives).





A PAGE FROM INGENHOUSZ' PRESCRIPTION BOOKLET. (*Courtesy* Breda Municipal Archives and Museum for the History of Science at Leiden).

commonly be observed; for the peculiar degree of goodness of the dephlogisticated air obtained from the leaves depends upon too many circumstances to be constantly of the same quality. Some more or less light of the sun thrown upon the jar will make some difference; the leaves being more or less crowded together, will make a remarkable difference, as a great number of them may be shaded from the sun by others.

As I made the greatest part of my experiments according to the present method of proceeding of my respectable friend the Abbé Fontana, it would have been difficult to imitate the experiments related in this work, and even to understand the manner in which they were made, if he had not given me leave to anticipate the publication of his own ingenious contrivance, and of his present method of putting the different species of air to the test. This kindness of that gentleman deserves my public thanks.

Inaccuracies in the manner of expressing myself will find some indulgence in a man born and educated in the Republic of the United Provinces, and who was not early in life acquainted with the English language.

The August Sovereigns, whom I have the honour to serve, condescending graciously to prolong my leave of absence, and allowing me to spend the present summer in this island, I thought it my duty to apply the time granted me by their goodness to an useful purpose, and to make all the advantage I could of that peculiar degree of health which I have always enjoyed in this climate.

On purpose to avoid every cause of obstructing my mind in the close pursuit of the object I had in view, and in tracing Nature in its operation on this subject, I disengaged myself from the noise of the metropolis, and retired to a small villa, where I was out of the way of being interrupted by any body in the contemplation of Nature.

This work is a part of the result of above 500 experiments, all which were made in less than three months, having begun them in June, and finished them in the beginning of September, working from morning till night. From these experiments some more consequences might have been drawn, if I had had more time to employ myself in a work upon such important matter. Whatever I have been able to deduce from my labours is done in a hasty manner, as my stay in this country was far too limited to allow me to compose my work in a regular and more satisfactory manner.

Though I was very far from foreseeing all the discoveries which I made in the course of this summer, yet I was persuaded that a good deal of the economy of the vegetable kingdom might be discovered by a steady pursuit of experiments tending to trace the operations of Nature. I had this object in view some years ago; but, as I did not enjoy such a favourable disposition of mind and body as was necessary for a task, in which all possible steadiness, perseverance, and close attention were requisite, I deferred the undertaking till I should find myself more fit for it.

Detached experiments may indeed be very useful when a sufficient number is collected to draw some conclusions from them; but, without pursuing methodically the same object, discoveries are to be expected only by mere chance, and are even sometimes overlooked. I owe to the example of my worthy friend, the Abbé Fontana, the thorough persuasion, which I now entertain, that natural knowledge can make but a very slow progress in the hands of those who have not patience and assiduity enough in pursuing one and the same object, till they discover some things undiscovered before; or till they find that the difficulty of the undertaking surpasses their abilities.

When this book was entirely printed, and nothing but the latter end of the preface unfinished, I was informed by my friend the Abbé Fontana, that he discovered a few days ago a new method of procuring to a sick person the benefit of breathing any quantity of dephlogisticated air at a cheap rate.

This very year a paper of mine was read before the Royal Society, and ordered for the press (containing a new theory of the effects of gunpowder, and the discovery of a new and powerful explosive air), in which I say, that the rapid progress our modern philosophers daily make on the different kinds of air, induce me to believe that we touch at the happy moment, at which a very easy and little expensive method of producing this beneficial fluid, in any quantity wanted, will be produced for the cure of many diseases.

I have the great satisfaction to inform the reader, that my prediction is fulfilled even before it hath reached the public, and that this important *desideratum* in medicine is discovered.

Abbé FONTANA found that an animal breathing-in either common or dephlogisticated air renders it unfit for respiration by communicating to it a considerable portion of fixed air, which is generated in our body, and thrown out by the lungs as excrementitious. This fixed air is easily absorbed by shaking it in common water, but infinitely more readily by the contact with quick-lime water.

He fills one of the large receivers of an air-pump, which are very wide at their upper extremity, half full of dephlogisticated air extracted from nitre, so that it may contain about 500 cubic-inches of this air, which will serve for breathing during half an hour. The manner of drawing this air out of the receiver, is either by thrusting a bended glass tube under the receiver (when this is floating in water, in which it is supported by its peculiar bulky form), reaching into the air itself, and keeping the other extremity in the mouth; thus drawing this air in the lungs, and breathing it out by the same tube. This air returning from the lungs is infected by fixed air, which being immediately absorbed by the contact with lime-water, the dephlogisticated air is restored very near to its former purity. Instead of the bended tube just mentioned, the top of the receiver may be made as the neck of a bottle, and a tube may be fixed to it, having a cock to open and shut the passage as required.

We consume, by each inspiration, about 30 cubic-inches of air; and thus, allowing 15 inspirations for a minute, we consume each minute 450 cûbic-inches of air. The Abbé FONTANA found, that the dephlogisticated air being, after each respiration, purified again by the lime-water, will remain good about thirty times as long as it would when breathed in the ordinary way; and that thus the quantity of dephlogisticated air necessary for one minute will now serve for breathing during half an hour, and thus the expences will be thirty times less.

We consume, in the space of 24 hours, about 21600 cubic-inches of air. One pound of nitre yields by heat about 12000 cubic-inches of dephlogisticated air, and thus it yields more air than any patient could consume by breathing this beneficial air the whole day (for we must allow at least 12 hours in the 24 for sleeping and necessary occupations), because this quantity will serve thirty times longer by the method explained, than in the ordinary way. It follows by this, that the expences required for breathing a whole day dephlogisticated air cannot amount to one shilling.

I have only just time enough to announce this happy discovery to the publick; whose great utility will, I trust, soon be found in the curing of inflammatory and putrid diseases, &c. in which too great a quantity of inflammable principle is let loose in our blood.

> I have also discovered, since my book was printed, that, in reading Dr. PRIESTLEY's last work, I had overlooked a remarkable passage, p. 270, in which he hints at what I found to be the case with inflammable air having stood a long while with plants. I think it my duty to relate his own words: "I am satisfied, however, from my own observations, that air may be very offensive to the nostrils, probably hurtful to the lungs, and perhaps also in consequence of the presence of phlogistic matter in it, without the phlogiston being so far *incorporated with it*, as to be discovered by the mixture of nitrous air."

EXPLICATION OF SOME TECHNICAL TERMS

As this volume may possibly fall into the hands of some who have not yet read the works of Dr. PRIESTLEY, and are entirely stranger to the new doctrine of air; I think it will be useful to explain the meaning of the new invented names given to different kinds of air mentioned in this book.

Nitrous air is that kind of *permanent elastic fluid* which is extracted by nitrous acid or *aqua fortis* from the most part of metals, principally from mercury, brass, copper, &c. This air is by a bended glass tube conducted under an inverted jar full of water. The nitrous air, thus disengaged, rises through the water, and settles at the inverted bottom of the jar. Mercury yields the strongest nitrous air, and always of the same quality; but heat must be applied for its extrication, if a greater quantity is in a short time required. I use for convenience sake copper, from which nitrous air is extracted in abundance in a short time, without applying heat. The nitrous acid must be diluted with water for the purpose.

Inflammable air is that air which rises up in bubbles from stagnant waters, whose bottoms are marshy, when the ground is stirred up with a stick. It is also extracted from iron, zinc, and some other metals, by diluted vitriolic or marine acid. This air has in common with almost all other inflammable substances, that it is not susceptible of actual inflammation, without it be in contact with common or respirable air. This air has the following qualities by which it may be known: it is not absorbed by shaking it in water; it is not diminished by the addition of nitrous air; it is instantly and absolutely mortal to animals breathing in it; it burns by the approach of the flame of a candle, where it is in contact with common air; but the whole of it inflames at once, and explodes with a loud report, when it is intimately mixed with common, and principally with dephlogisticated air.

Phlogisticated air, is, properly, air impregnated with phlogiston, or with the inflammable principle. It has received this name because common air, exposed to the calcination of metals, becomes phlogisticated air; which alteration seems to depend on the phlogiston of the metal being communicated to it, for the metal itself has lost it in the calcination; and because the burning of a candle, and many other phlogistic processes, impart to common air this quality. The air returning from our lungs is said to be phlogisticated more or less, because it is found to possess the same qualities as the air exposed to the calcination of metals. This kind of air is known by the following properties: It is not absorbed by water; it is not, or not much, diminished by nitrous air; it is poisonous for an animal who breathes in it; it is not inflammable either by itself or by the addition of respirable air, but extinguishes flame.

Dephlogisticated air, is that pure, etherial, permanent, and elastic fluid which the celebrated Dr. PRIESTLEY found out the first, and gave this very well adapted name to it. It is respirable air, destitute of the phlogistic or inflammable principle with which the best atmospheric air is found always to be more or less contaminated. It is in reality common air of the highest purity, such as never exists in the common atmosphere. It is even so far superior in purity to common air, that an animal shut up in a vessel, filled with this air, protracts its life four or five times, nay even in some cases seven times longer, than if it was shut up in a vessel filled with the best atmospheric or common air. Some of its qualities, by which it may be known, are the following; it is not absorbable by the contact of water; the flame of a candle plunged in it becomes larger, and of the most admirable brilliancy, so as to dazzle the eyes; red-hot charcoal plunged in it becomes shining and sparkling; it is much more diminished by nitrous air than common air; it explodes, with an uncommon loud report, when mixed with a certain proportion of inflammable air, or when a few drops of good vitriolic aether are poured in a vessel containing this air, as I discovered.

Fixed air is that kind of aerial fluid which issues in abundance from fermenting substances, and which, in some places, rises out of the ground by itself, as in the famous *Grotta del Cane* near Naples. It is this air with which some mineral waters are impregnated, and to which they owe their pungent taste and their virtue, as, for

instance, Seltzer waters. It is that air which arises in abundance from calcarious stones, by the addition of vitriolic acid. This air may be known by the following properties: it extinquishes flame; it is absorbed by water, and communicates to it the same pungent, acidulous taste as Seltzer water has, so as not to be distinguished from it either by the taste or by its virtues; it precipitates quick lime from water; it immediately crystallizes *oleum tartari per deliquim,* when put in a vessel lined with this oil; it is mortal to animals breathing in it.

Eudiometer, is a new word; it signifies an instrument by which we may judge of the degree of salubrity of the common air. The invention of such an instrument belongs to Dr. PRIESTLEY. It consists chiefly of a glass tube, divided in equal parts; for instance, in two large divisions; each of which is divided into ten others, and each of these ten sub-divided again into ten parts: and a glass measure, containing exactly one of the great divisions of the tube. One measure of common air and one of nitrous air, put together in a separate glass vessel, and left by themselves till the diminution of the bulk of the two airs is compleated, and afterwards let up in the glass tube, indicates at once the exact diminution of the two joint measures. The degree of goodness of the common air is found to be in proportion to the diminution of the bulk of the two airs. Mr. MAGELLAN, F. R. S. has published a work upon an ingenious contrivance of his own of this kind, which is to be sold by Mr. PARKER in Fleet-Street, with the proper directions how to use it. What considerable improvements we owe in this regard to Abbé FONTANA, will appear in the introduction to the second part of this work.

ADVERTISEMENT

As the author intends to publish a French translation of this work, he thinks it his duty to give public notice of his intentions, that no one may give himself any unnecessary pains about it.

For the Table of Contents which follows in the original edition, vide page 309.

(Legends for figures 1-11, page 322, abbreviated)

FIG. 1.— Apparatus for generating nitric oxide. (*a*) Small flask completely filled with nitric acid in which there are some pieces of copper wire. (*b*) A curved glass tube, one end of which was ground to fit the neck of the flask *a*, the other inserted in the neck of *c*, an inverted glass receiver containing water. The trough containing the apparatus should receive sufficient water to stand above the mouth of the glass receiver *c*.

FIG. 2.— (*a*) A glass tube equipped with the movable scale *cc*. The internal diameter of the tube should be about half an inch, and its length from fourteen to eighteen or twenty inches. It is fastened in the scale, in a way which permits it to be suspended and yet allows one to slide it to any desired point on the scale. It is well to line the inside of this part with a piece of finely minced sponge, and to prevent the copper (sleeve) from damaging the glass tube. Some holes, which one may see in this piece, allow for the passage of a wire to hold the sponge. Other holes may be seen in the upper part of the scale for lining the interior with a piece of finely cut sponge. The upper end of this glass tube is hermetically closed. The lower end is open and reenforced with a copper tube.

This tube is divided into several equal parts of about three inches each marked with a file or a diamond. Each of these divisions is subdivided into a hundred equal parts which are marked on the copper scale.

FIG. 3.— The eudiometric tube suspended in a copper tube *aaaa*. The glass tube is mounted in the movable scale which is supported with three pivots on the ring *bb*.

FIG. 4.— The part of the graduated scale separated from the lower part (Fig. 5) which bears the three pivots.

FIG. 6.— The copper ring on which the three pivots (*bb*, Fig. 3) rest.

FIG. 7.— The measure in its copper setting.

FIG. 8.— The assemblage of parts of the measure. *a* is the glass measure; *b*, a fitting which receives the measure; *c* is the mounting which receives the slide *e*, it is made of two sheets of copper forming a slot in which the slide *e* moves freely; *d* is the funnel shaped base of the mounting.

FIG. 9.—The copper mounting (Fig. 8, *c*) as seen from below.

FIG. 10.— A movable copper infundibulum which may be fixed at any tub or water level.

FIG. 11.— The tub which is used for eudiometric experiments. The internal diameter is two feet and filled with water to two inches from the rim. *a* is the plate, 9 inches wide and 2 inches thick, on which the different vases employed are placed; it ought to be fixed in the tub 3¾ inches below the rim. *d* is an infundibulum of glass set in a circle of copper and joined to the plate *a* with a piece of copper in a way that it may be easily removed. *cc* are hollows (grooves) 1½ inches wide and 2 or 3 inches long which accommodate the curvature of tubes through which gases may be passed into the receivers.

Redrawn from* INGENHOUSZ' *Plates

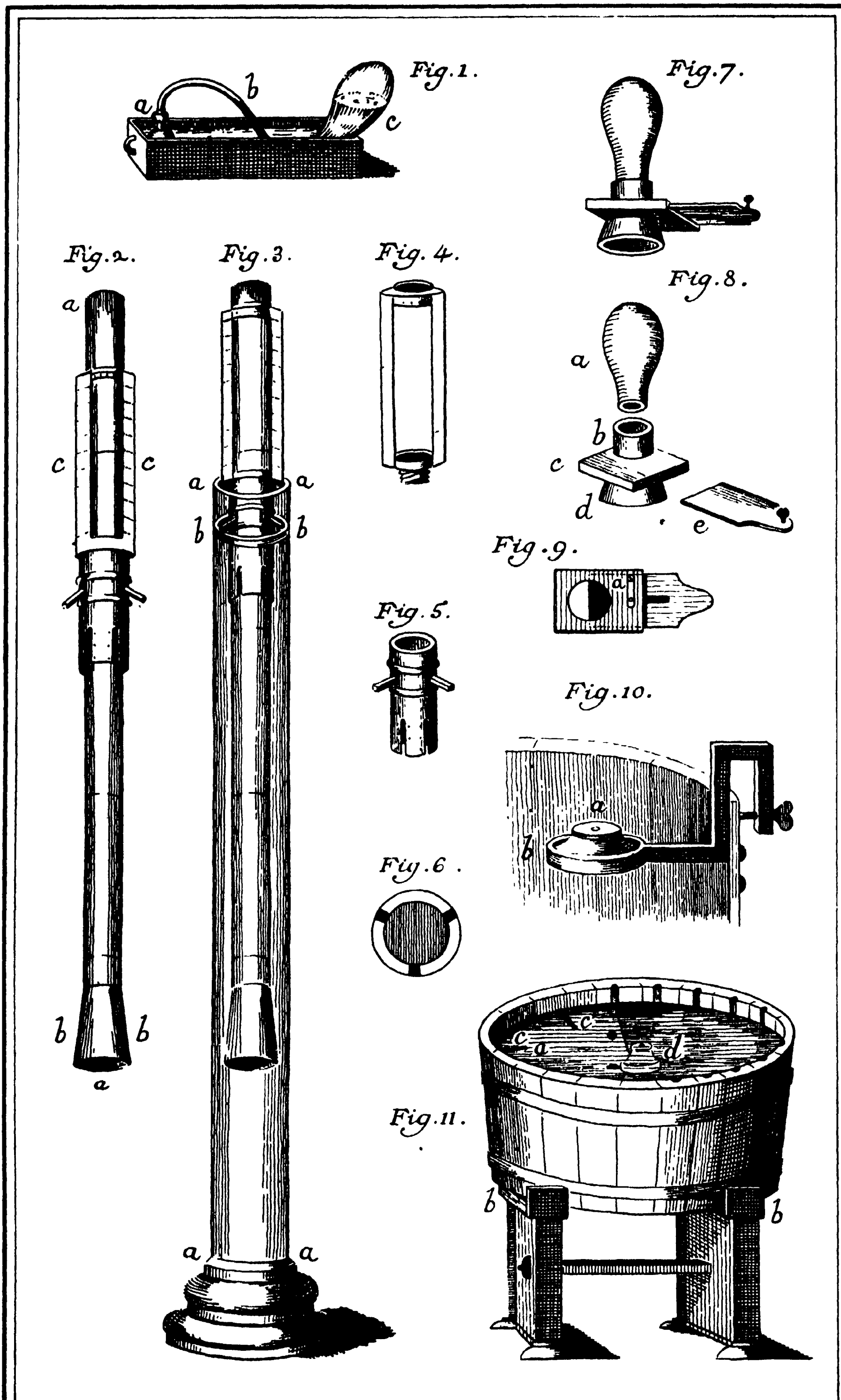


Fig.1.
a
b
c
Fig.7.
Fig.2.
Fig.3.
Fig.4.
Fig.8.
a
b
c
d
e
Fig.9.
a
Fig.5.
Fig.10.
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Fig.6.
Fig.11.
c
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Section I

SOME GENERAL REMARKS ON THE NATURE OF THE LEAVES OF PLANTS, AND THEIR USE

It seems to be more than probable, that the leaves, with which the most part of plants are furnished during the summer in temperate climates, and perpetually in hot countries, are destined to more than one purpose. Such a great apparatus, which nature displays as soon as the sun begins to afford a certain degree of warmth upon the surface of the earth, can scarcely be considered as solely destined either to ornament, to nourishment of the plant, to its growth, to ripen its fruit, or for any other peculiar and single use. It seems probable, that they are useful to the growth of the tree; for, by depriving the tree of all its leaves, it is in danger of decay. By taking a considerable part of the leaves from a fruit tree, the fruit is less perfect; and by taking them all away, the fruit decays and falls before its maturity. It is also probable, that the tree receives some advantage from the leaves absorbing, by their means, moisture from the air, from rain, and from dew; for it has been found a considerable advantage to the growth of a tree, to water the stem and the leaves now and then. But I leave the discussion of those articles to others, who have made these considerations an object of their pursuits. The late Mr. BAKER has published to the world his microscopical observations on the subject. Mr. BONNET, of Geneva, has published a very elaborate work upon the same, entitled, *Recherches sur l'usage des Feuilles dans les Plantes, et sur quelques autres Sujets relatif à l'Histoire de la Végétation, par* CHARLES BONNET, *à* Gottingen *et* Leiden, 1754. This work contains a great deal of interesting inquiries upon the nature, properties, and utilities of those wonderful organs; all of which have been treated with the greatest attention, and have thrown much light upon this subject.

This celebrated author has taken a great deal of notice of those air bubbles which cover the leaves when plunged under water. He says, p. 26, that the leaves draw these bubbles from the water. He is the more persuaded that this is the case, because he found these bubbles did not appear when the water had been boiled some time, and appeared more when the water is impregnated with air, by blowing in it. He had also observed, that they did not appear after sunset. Page 31, he explains his opinion farther upon this head: he says, that these air bubbles are produced by common air adhering to the external surface of the leaves, which swells up into bubbles by the heat of the sun; and that the cold of the night is the reason why these air bubbles do not make their appearance at that time. As he found that dry leaves put under water gather such bubbles also upon

their surface, he concludes, p. 33, that the appearance of these bubbles cannot be owing to any vital action in the leaves.

I took some pains to disclose the cause of these bubbles, which, I think, are of more importance than Mr. BONNET at that time imagined them to be; and found the fact to be generally this:

The most part of leaves gather these bubbles upon their surface, when plunged in any water in the sun-shine or by day-time in the open air; but infinitely more in fresh pump water than in any other. In clear river water they appear later, less in number and in size; less so in rain water, and the least of all in boiled water, in stagnating, and in distilled water.

They are not produced by the warmth of the sun rarifying the air adhering to the leaves; for many kinds of leaves produce them almost as soon as plunged under water, though the water be very cold, and the leaves warm from the sun-shine be plunged in it.

They do not appear after sun-set, at least not in any considerable number; but those that already exist do not shrink in or disappear by the cold of the night.

As soon as the sun begins to diffuse its warmth over the surface of the earth in the spring, and to promote that general tendency to corruption which all dead bodies of the animal and vegetable kingdom, and many other substances, are so liable to, the trees display in a few days the most wonderful scene that can be imagined. Contracted as they were in that state of stupor and inactivity in which they remain during the winter, exposing to the air no other surface than that of their trunk and branches, as if they wanted to have as little to do as possible with the external air, they all at once increase, perhaps more than a thousand times, their surface by displaying those kind of numberless fans which we call leaves. Some of them produce their leaves a long while before any flowers appear upon them; others a good while after the flowers are formed, and the fructification is already in an advanced state; and keep their leaves in the best condition, and even push out continually new ones, long after the whole fructification is finished; which seems to indicate, that the chief use of these fans is not to assist the fructification and propagation of their species. These fans, when compleated, seem to compose or arrange themselves in such a manner as to expose their upper and varnished surface to the direct influence of the sun, and to hide as much as they can their under surface from the direct influence of this luminary. It seems as if they required rather the light of the sun than the influence of its heat, as their polished surfaces must reflect some of the rays of the sun, and thus moderate the degree of heat.

It will, perhaps, appear probable, that one of the great laboratories of nature for cleansing and purifying the air of our atmosphere is placed in the substance of the leaves, and put in action by the influence of the light; and that the air thus purified, but in this state grown useless or noxious to the plant, is thrown out for the greatest part by the excretory ducts, placed chiefly, at least in far the most part of plants, on the under side of the leaf.

Is there not some probability that the under part of the leaves may have been chiefly destined for this purpose; because in this way the dephlogisticated air, gushing continually out of this surface, is inclined to fall rather downwards, as a beneficial shower for the use of the animals who all

breathe in the region of the air inferior to the leaves of trees? Does not this conjecture get some weight, if we consider that dephlogisticated air is in reality specifically heavier than common air, and thus tends rather to fall downwards?

If we add to these reflexions another of no less importance, *viz.* that most sorts of foul air are specifically lighter than common air, we shall be inclined to believe that the difference of the specifical gravity of that beneficial air of which I treat, and that which is become hurtful to our constitution by corruption, breathing, and other causes, indicates one of those special blessings designed by the hand of God: for by this arrangement we get soon rid, in a great measure, of that air which is become hurtful to us, as it rises soon up out of our reach; whereas the dephlogisticated air, being heavier than common air, is rather inclined to settle on the surface of the earth among the animal creation.

But, as animals spoil equally as much air in the winter as in the summer by the act of respiration, it might seem somewhat surprizing, that this great laboratory ceases entirely by the decay of the leaves. Is this defect supplied by some other means equally powerful? Though we are very far from being able to trace all the active causes which contribute their share in keeping up the wholesomeness of our atmosphere, yet we have already traced some of them, and therefore must not despair of discovering some more. The shaking of foul air in water will in great measure correct it. Water itself has a power of yielding dephlogisticated air, as Dr. PRIESTLEY discovered. Plants have a power to correct bad air, and to improve good air. Winds will blow away the noxious particles of the air, and bring on air corrected by the waters of the seas, lakes, rivers, and forests. All these causes exist equally in the winter as in the summer, or at least nearly so. The influence of the vegetable creation alone ceases in the winter: but the loss of this influence is, perhaps, more than amply counterbalanced by the diminution of the general promoting cause of corruption, *viz.* heat. Every body knows, that warm weather hastens in a great degree putrefaction. In the summer time numberless insects are produced, which did not exist in the winter: these insects infect the air by the corruption of their bodies. That immense quantity of animal substances, and many others, which undergo a putrefaction by the warmth of the weather, seems to require an additional power or agent to counter-act it; and this office is destined to the leaves. In frosty weather no animal substance is subject to putrefaction, which cannot go on without a proper degree of heat. The perspiration of animals is less offensive in the winter than in the summer, and of consequence must corrupt the atmosphere less. It seems therefore probable, that, if we are deprived of one way by which air is corrected in the winter, we have also at that time less causes which tend to contaminate our element.

Comments on Section I, Global Significance of Photosynthesis:— INGENHOUSZ expressed in many ways his conviction, often implicit, that the action of light on green leaves has cosmic significance. In his quaint phraseology he revealed an insight into the order of nature which few of his contemporaries had expressed. He speculated profitably on the cycles of carbon dioxide and oxygen in the air and waters of our planet, though he lacked adequate scientific data for precise statements. We now know that some of his ideas are untenable, yet we may notice a few recent advances and try to relate them to earlier notions of the action of living plants on the cycles of carbon and of oxygen.

INGENHOUSZ had some original, but not entirely correct ideas about the sources of carbon dioxide in the atmosphere. He knew that it was liberated from plants when they were not illuminated and from nongreen parts of plants when illuminated. He also knew that it was exhaled by soils, by fermenting materials, by volcanic activity, and that ground waters contained varying amounts. He realized, to a certain extent, that natural waters are a reservoir of carbon dioxide, but in his day it would be difficult for anyone to visualize the state of equilibrium which must exist between the atmospheric carbon dioxide and the vast quantity present in the oceans. The amount of oxygen liberated into the atmosphere of the earth and the cycle of oxygen in nature in which green plants play such an important role were discussed in several passages in the book. His interest was admittedly aroused by considerations of human physiology, but he realized that the subject had wide implications.

Because of many broad scientific concepts involved, the reader may be interested to consider a brief discussion of the global aspects of photosynthesis, though the results of quantitative studies are indeed scanty.

The earth's atmosphere contains about .03 per cent carbon dioxide, about 21 per cent oxygen, and about 78 per cent nitrogen. The data of geology indicate that the volcanic activity of our earth has varied greatly in times past. During periods of great activity the carbon dioxide of the air would be more abundant than in periods of quiescence. There is now geochemical evidence that most of the oxygen liberated into the atmosphere is contributed by marine plants, though additional information concerning the ultimate origin of this oxygen is needed for comparisôn with results obtained with fresh water and land plants (KAMEN and BARKER, 1945). The present rate of photosynthesis is sufficient to produce the quantity of oxygen in the atmosphere in a few thousand years and to maintain the present amount. It has been calculated that the atmosphere of the earth contains approximately 2.8×10^{14} tons of oxygen, and that living creatures consume annually the equivalent of 15×10^{10} tons of carbon dioxide which would contain 12×10^{10} tons of oxygen.

Some pertinent figures on the carbon turnover are given by H. GAFFRON, in the article on photosynthesis in the last edition of the Encyclopaedia Britannica, in which he estimated that the quantity of carbon dioxide reduced per year is equivalent to 10^{11} tons of carbon, more than four-fifths of which is contributed by the flora of the oceans. At that rate the total quantity of carbon dioxide in the atmosphere would pass through the life cycle in about 350 years. GAFFRON also pointed out that it is quite likely that the atmosphere of the earth, when it fell below a temperature of 100° C., contained little or no free oxygen, but plenty of carbon dioxide. If this be granted, then our entire supply of oxygen, one fifth of the atmosphere, has been furnished by photosynthesis.

The cycle of carbon assimilation requires also oxygen and hydrogen both of which are available in larger quantities than carbon. At the previously mentioned rates it would take about 2000 years to renew all the oxygen in the air and 2,000,000 years to decompose all the water on our planet. Several biochemists have pointed out that life cannot have started with this particular synthesis because the chlorophyll system upon which it depends is highly complicated and is, itself, a product of photosynthesis.

Estimations of the amount of the annual production of organic carbon on the earth are, at best, approximations based on determinations made independently at various sites. It is nevertheless important to consider the total yield of photosynthetic products, because, as shown above, the process of formation liberates oxygen, on which life is dependent.

Forests and farmlands are obviously the most important land areas for the production of organic carbon, while the large areas of steppes and deserts produce relatively less. The oceans possess a power for carbon assimilation whose magnitude is often unappreciated. The organisms which compose the phytoplankton can function only a few meters below the surface, but, on account of the relatively uniform temperature and the constancy of the supply of water and carbon dioxide, they work with an efficiency which is a contrast with land plants, which have periods of inactivity due chiefly to climatic conditions. The large reservoir of dissolved carbonates and bicarbonates in the ocean is in equilibrium with the gaseous carbon dioxide in the atmosphere. Furthermore, the plankton has no transpiration, hence utilizes all the solar energy for photosynthesis, whereas land plants use most of the solar energy which they receive for transpiration. *Fucus serratus* shows a maximum of carbon assimilation at 5° C. (HYDE, 1938) and a secondary maximum at 25° C.

The light and temperatures of subsurface oceanic waters are seldom optimum for oxygen production by diatoms (BARKER, 1935), yet, the total amount they produce is significantly great. It may be assumed that light is the factor which has the greatest influence on carbon assimilation of plankton. From results obtained in bottles, RILEY (1941) estimated that in free water of Long Island Sound, the amount of organic carbon fixed by photosynthesis varied from .02 to .41 g. per m^3 per day, with a mean value of .175 g. On Georges Bank oxygen production began to increase rapidly in March (RILEY, 1941*a*), reached a high point in April, and decreased in the following months.

RABINOWITCH (1945) gave this suggestive table of values obtained from various determinations.

CARBON FIXATION BY LAND AND SEA PLANTS: —

Plant Habitat	Area, millions of sq. km.	Average carbon fixation per hectare per year	Total carbon fixation per year
		tons	tons
Oceans	361	3.75	15.5×10^{10}
Land	149	1.3	1.9×10^{10}

The photosynthetic activity of the plankton was positively correlated (RILEY, 1941) with the amount of chlorophyll, the temperature, and the light; it was negatively correlated with the amount of zooplankton. The entire plankton association consumed about half the oxygen produced at the surface.

With a greater appreciation of the energetics of the atmosphere and terrestial creatures, we have explored more deeply the concepts which INGENHOUSZ formulated about oxygen cycles in his pioneer book, learning that the power of the green plant to use solar energy to reduce carbon dioxide and to produce compounds richer in energy is one of the tremendously important biological processes on this planet.

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Section II

ON THE MANNER IN WHICH THE DEPHLOGISTICATED AIR IS OBTAINED FROM THE LEAVES OF PLANTS

As the leaves of plants yield dephlogisticated air only in the clear day-light, or in the sun-shine, and begin their operation only after they have been in a certain manner prepared, by the influence of the same light, for beginning it; they are to be put in a very transparent glass vessel, or jar, filled with fresh pump water (which seems the most adapted to promote this operation of the leaves, or at least not to obstruct it); which, being inverted in a tub full of the same water, is to be immediately exposed to the open air, or rather to the sun-shine: thus the leaves continuing to live, continue also to perform the office they performed out of the water, as far as the water does not obstruct it. The water prevents only new atmospheric air being absorbed by the leaves, but does not prevent that air, which already existed in the leaves, from oozing out. This air, prepared in the leaves by the influence of the light of the sun, appears soon upon the surface of the leaves in different forms, most generally in the form of round bubbles, which, increasing gradually in size, and detaching themselves from the leaves, rise up and settle at the inverted bottom of the jar: they are succeeded by new bubbles, till the leaves, not being in the way of supplying themselves with new atmospheric air, become exhausted. This air, gathered in this manner, is really dephlogisticated air, of a more or less good quality, according to the nature of the plant from which the leaves are taken, and the clearness of the day-light to which they were exposed.

It is not very rare to see these bubbles so quickly succeeding one another, that they rise from the same spot almost in a continual stream: I saw this more than once, principally in the *nymphaea alba.*

Comments on Section II:— The reader who is interested in modern ideas of photosynthesis which were implicit in INGENHOUSZ' work will find many excellent discussions in recent literature. The limitation of space prevents the present writer from making more than a few brief references to present day advances. Nevertheless, a few fragmentary, but hopefully pertinent observations will be inserted.

All the knowledge since obtained rests securely on the solid foundation which INGENHOUSZ made when he announced that, "The light of the sun alone is capable of producing in the leaves that activity which may produce dephlogisticated air: as soon as the light ceases to act upon the leaves, their activity simultaneously ceases, and another of a different nature commences."

INGENHOUSZ appreciated the necessity for chlorophyll in the photochemical processes, although he knew little about the exact nature of the pigment. CAVENTOU and PELLETIER isolated and gave to the compound the name which it now bears, but many years passed before the appearance of any sound ideas on the mechanism of photosynthesis. A prodigious amount of work was done on the subject without, however, elucidating the phenomenon. The subject was clarified and tremendously simplified by the postulate of VAN NIEL (1941) that photosynthesis may be considered as a

coupled oxido-reduction comparable to other reactions of this kind requiring a hydrogen donor. Essentially, the process may be represented by the equation

$$CO_2 + 2\,H_2A \xrightarrow{\text{light}} (CH_2O) + H_2O + 2A$$

in which H_2A, the H-donor, is represented by water. In the photosynthetic process of sulphur bacteria, a variety of simple organic substances may serve as hydrogen donors. (For additional discussion *see* comments on Section V).

In another special section, INGENHOUSZ attacked the question whether it is the warmth or the light of the sun which causes the evolution of oxygen from green leaves. His formulation of the problem was very clear and unmistakable for the state of physics in his day. He undertook to study the question whether heat which was independent of the radiation of the sun could be as effective as the illumination received from the sun and coming principally in daylight. He concluded as follows: "When the sun expands the air, as a result of the heating of the water, it would cause that the dephlogisticated air should develop from the leaves immersed under water. It might follow that, if a leaf which became heated by the noonday sun were plunged suddenly into very cold, fresh spring water, air bubbles could not be given out until the water had received at least a small amount of heat. Alone, it happens exactly the opposite. The leaf which one takes from a tree or an herbaceous plant at noon on a sunny warm day and dips immediately into cold spring water forms with especial rapidity air bubbles which consist of the purest dephlogisticated air. Were it due to the heat, and not to the sunlight, one could obtain similar air if he would heat the water by means of a fire to almost the same degree of temperature that obtains in the sunshine. This is, however, generally not the case."

"I dipped leaves in a glass vessel, under water, stood the vessel inverted over spring water and brought the apparatus so near the fire (by weak illumination) as was necessary in order to cause, in the vessel, a measured amount of heat of the same degree as a similar vessel with leaves of the same kind at the same time in free air received from the sun. The result was that the air liberated by the heat of the fire was very badly vitiated. On the contrary, the air obtained in the sun consisted of pure dephlogisticated air."

The experiments were varied in another way. The result was always the same. Also, in the open air, raising the temperature, independent of the radiation of the sun, liberated no oxygen, and likewise, in a heated room, if the daylight was not sufficiently strong, no oxygen was evolved by the green plant. INGENHOUSZ came to the conclusion that, "One could also determine easily why Mr. SCHEELE came to the conclusion that the air in the vessels was vitiated by the beans growing in them. These men unfortunately expected the aforementioned wholesome effect from the growth of the plant. If one lets a plant stand day and night in a vessel filled with ordinary air, the outcome of the experiment will depend upon the degree of illumination the plant receives." (WIESNER, 1905, page 69).

After many experiments undertaken with various plants and plant organs, it was always shown that only the green parts, especially the leaf blade, were capable of liberating oxygen in the light. On the contrary, all of the other nongreen plant organs, either in light or in darkness, liberated vitiated air as also did the green plant in the darkness. INGENHOUSZ was led to reflect upon the matter and to express himself in somewhat the following way: "The plants seem to draw the greatest part of their sap from the earth by means of the roots expanded in all directions. They obtain their phlogistic nourishment, on the contrary, principally from the air. They elaborate this air in the substance of their leaves. They separate from it those substances which are appropriate for their nourishment, namely, those having a combustible character, and the remainder is ejected like matter which is harmful to them. The materials mentioned are, however, wholesome for animals. This theory seems to be supported, both by the relationship and through the natural phenomenon itself. It throws much light on the arrangement and organization of nature and on the opposing influence of the vegetable and animal kingdoms. It has a specific connection with various other generally and sufficiently well known natural phenomenon. A plant is a living organism without movement and remains during its entire lifetime in the place of its origin, incapable of searching out its nutrients after the manner of animals, it must find all the needs for its life, and for its maintenance, where the Creator determined it, in the narrow confines of its location. They must have uncounted root fibers in the soil on

which they grow, in order to obtain through them the sap for conduction to the upper part of the tree. These root fibers must reach out and bring in all of the things which the tree needs in this time. Since the tree, in the summer, has greater activities to carry on, so it brings forth at this time innumerable leaves, which, in the most advantageous manner, are so placed that they shade each other as little as possible or interfere with the inhalation of the atmospheric air." (Cf. Section XVIII, p. 361.)

"While they are acquiring, in this way, a part of the atmospheric air, they are simultaneously presenting, if I may say so, this substance drawn from the atmosphere to the direct rays of the sun, in order that it may be repurified by the influence of that great luminary and again brought to the highest degree of perfection" (WIESNER, page 71).

The possibility that plants may derive a varying amount of the intermediate substances of carbohydrate metabolism from sources other than the air must be considered before the subject is closed. During the decomposition of organic matter in the soil by microorganisms many compounds are formed (in addition to carbon dioxide). We must by no means overlook the possible effect, in this connection, of the mycorrhizal associations of certain fungi with the root extremities of many plants, though their functions are not yet perfectly understood. REED and MACDOUGAL demonstrated (1945) that *Corallorhiza*, a chlorophylless plant, could grow to maturity in darkness as well as in light, obtaining the substrates for growth through the activity of its fungal associates. The important point is that the substrates for growth were passed on to the plant from the fungal associates.

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Section III

THE MANNER IN WHICH THE DEPHLOGISTICATED AIR OOZES OUT OF THE LEAVES IS DIFFERENT IN DIFFERENT PLANTS

It is somewhat amusing to observe the constant manner in which the dephlogisticated air makes its appearance upon the same kind of leaves, and the surprizing difference which takes place in the leaves of different plants. Some leaves, for instance, form always small round bubbles, as is the case with the most part of leaves; others form irregular flat blisters, as do the leaves of the honeysuckle, *caprifolium*. Some, and indeed the greatest part, produce round bubbles on both surfaces; others yield on one surface round bubbles, on the other irregular blisters; for instance, leaves of oak, which give flat blisters on the under side, and round bubbles on the upper side. Some form only those irregular blisters at the upper side, as the leaves of spurge, *cataputia* or *euphorbia*.

Some leaves form neither bubbles nor blisters on either side, and yet yield a great deal of dephlogisticated air; for instance, leaves of *nasturtium Indicum:* these leaves seem to have a quality repulsive to water, which only forms a kind of cover over the surface of the leaves, without coming into immediate contact. The air oozing out of the leaves floats under this cover, and rises to the highest part of the leaves, where it forms a kind of bags, which at last detach themselves from the edge, and rise to the top of the jar. The leaves themselves, after standing a day and a half in the water, are not wetted by it, but come out quite dry.

Some leaves have that peculiar quality of being wetted by the water only on one side; as, for instance, leaves of raspberry shrubs, which do not receive the wet on their shaggy under surface.

Strawberry leaves repel the water from both surfaces, form blisters at the under surface, and chiefly round bubbles at the upper surface.

Some leaves begin very early in the morning to yield dephlogisticated air, and cease late in the evening; for instance, potatoe and *malva* leaves.

Some begin this operation very late in the morning, and cease very early in the evening; for instance, leaves of *laurocerasus*.

Some leaves yield the air bubbles immediately, as leaves of potatoe plants; some in a few seconds, as *malva;* some in a few minutes, as walnut trees; some much later, as leaves of *laurocerasus*.

Some yield the air bubbles first on the under side, as almost all leaves of trees; some first at the upper side, as leaves of *laurocerasus;* some on both surfaces at the same time, as *malva*.

On some leaves the air bubbles grow almost all regularly one with another, as in leaves of a vine, walnut, lime tree; in some they are, from the very beginning, of a very irregular size, as in *malva,* parsley, &c.

These few instances shew the various ways in which this beneficial air is oozing from the leaves, and which can only be owing to the different organization of the leaves in different plants.

I have discovered a good deal more of such remarkable peculiarities among leaves of different plants; but those above mentioned will be sufficient to shew, that each plant follows in this regard its own nature; and that, therefore, these different appearances seem to depend upon some vital motion going on in the substance of the leaves.

Section IV

THE DEPHLOGISTICATED AIR OOZING OUT OF THE LEAVES IN THE WATER IS NOT AIR FROM THE WATER ITSELF

The reverend Dr. PRIESTLEY found, that water, chiefly pump water, standing some days by itself, forms at the bottom and sides of the vessel a kind of green matter, seemingly vegetable, from which air bubbles rise continually to the top of the jar, if exposed to the sun-shine: that this air is fine dephlogisticated air, which shews that there is a faculty in water to produce by itself this beneficial fluid; and thus, that the mass of the waters of the sea, lakes, and rivers, have their share in purifying the atmosphere.

But as this dephlogisticated air is not produced immediately from the pump water, but only when this green matter is formed, it is clear, that the air obtained from the leaves, as soon as they are put in the water, is by no means air from the water, but air continuing to be produced by a special operation carried on in a living leaf exposed to the day-light, and forming bubbles, because the surrounding water prevents this air from being diffused through the atmosphere.

It is true, that pump water, placed in the sun-shine, will soon yield some small air bubbles, settling at the bottom of the jar, and every where at the sides; but this air is very far from being the same as that contained in the air bubbles of the leaves.

I placed, in a warm sun-shine, a great number of inverted jars, full of pump water, and collected carefully from them all the air yielded by these bubbles, which proved to be much worse than the common air.

I boiled some pump water in a pot, in which I had placed a long cylindrical jar, quite full of the same water: a good deal of air was collected at the top of the inverted jar, which was by the heat disengaged from the water. This air proved to be much worse than common air, and entirely unfit for respiration.

Abbé FONTANA has made, some years ago, a great many experiments, tending to investigate the nature of air contained in different waters.

Comments on Section IV:— INGENHOUSZ was wrestling with a problem which confronted investigators of his time, and was never quite satisfied with his conclusions. We can admire, however, the way in which he tackled the question of the origin of the gas produced in these simple experiments. Not many of his contemporaries were so sapient. It will be instructive to cite one or two instances.

Count RUMFORD, being dissatisfied with INGENHOUSZ' conclusions with respect to the origin of the air produced from plants under the influence of the sun's rays, proceeded to make experiments with non-living substances. He filled transparent glass flasks with spring water, into which he introduced raw silk, eider-down, hair, linen and other substances. He reported that, when the flasks were exposed to light, air bubbles soon appeared on the surface of the silk, growing in number and size, and rising to the top of the flasks. When the gas was collected and analysed it had the properties of dephlogisticated air. With naïveté RUMFORD said that after four days the water in the globe lost something of its transparency changing to "a very faint

greenish cast." When a similar experiment was made in darkness a few air bubbles were formed on the silk, but none arose to the top of the globe (*vide* RUMFORD's Collected Works. Publ. London, 1876).

He demonstrated that more dephlogisticated air was produced in strong than in weak light, yet he was curiously oblivious to the function of the green substance. He wrote, "Thus in speaking of the air produced upon exposing raw silk in water to the action of light, I shall sometimes mention it as being yielded by the silk; and I shall sometimes speak of the air furnished by exposing water, which has previously turned green in the sun's rays, as being immediately produced by the water, though it is probable that the *green matter* acts a very important part in the production of this air in the one case and perhaps in the other. But how it acts is not well ascertained; and I had in general much rather confine myself to a simple and even unlearned description of facts, than, by endeavouring to give more precise definitions, at first, to involve myself in all the difficulties which would attend an attempt to account for phaenomena whose causes are very imperfectly known."

Had RUMFORD known about PRIESTLEY's discovery of the "green matter" in earlier experiments, he would have avoided this (to us) comical mistake. It is not known whether RUMFORD ever acknowledged his error.

There is evidence that, following the publication of BONNET's work, there was no little discussion of the source of the gases liberated from leaves submerged in water. An excerpt from the French edition of his book shows that INGENHOUSZ continued to cogitate on the subject. He re-examined the pioneer work of PRIESTLEY and was convinced that the dephlogisticated air was produced only by "green substance or by leaves."

> "PRIESTLEY believes (vol. 4, 1779) that that green substance was neither plant nor animal, nor could it be one or the other. It was only after the publication of my work in English, printed in the same year, in which I inferred that it was a kind of moss (an error, as one may see, in my memoir published in the Journal de Physique of July, 1784) that he (PRIESTLEY) started to classify it among plants; but this was only after my friend BEWLEY had examined a species of conferva, mistaking it for the green substance which PRIESTLEY describes in his fourth volume as a viscous substance, without any apparent organization. BEWLEY would not have fallen into this error if he had consulted the work itself and compared the description which I have just cited with the plant which he took for the green substance, which is mentioned in the work of Dr. PRIESTLEY.
>
> I do not know whether one may pretend with certitude that dephlogisticated air thus obtained from the water, after that green substance appeared in it, is a gas arising from the water. However that might be, the case is not applicable to that of leaves of plants plunged under water, for in that case the green substance requires some days before the production of dephlogisticated air takes place, which indicates that it is not the water, but the green substance, which produces that gas."

INGENHOUSZ evidently believed that water is a simple substance although some of his observations seem clearly to indicate the decomposition of this fluid.

SENEBIER (1788) also tackled the problem of the source of gases liberated from leaves submerged in water. He set up series of tests in which carbonated water was compared with distilled and with boiled water, and with mixtures of the same. The amounts of gas evolved by the leaves were quantitatively determined. SENEBIER was clear in his arguments and concise in his statements that the leaves used the carbon dioxide in the water to produce the gas evolved. Where no carbon dioxide was present, the leaves evolved no gas. When leaves produced gas, the impregnated water lost its carbon dioxide. The amounts of gas evolved by the leaves in impregnated water when exposed to the light were so abundant, surpassing in volume that of the leaves which produced it, that we are obliged to admit that this gas furnished by the leaf is a gas which it has received from the water and is, up to a certain point, proportional to the amount of fixed air (carbon dioxide) contained in the water.

THÉODORE DE SAUSSURE (1804), reaping the harvest of earlier workers, more successfully elucidated the matter. His conclusions are of sufficient scientific and historic importance to be included herewith. *A propos* of the decomposition of water during the photosynthetic process, he wrote (chapter 7):

"Plants appropriate to themselves the oxygen and the hydrogen of water, thereby making it lose the liquid state. This assimilation is only well marked when they simultaneously incorporate carbon into themselves. . . . But the plants, in any case, do not directly decompose water in assimilating to themselves its hydrogen and in eliminating its oxygen in the form of gas; they only exhale the oxygen gas by the immediate decomposition of the carbonic acid gas."

DE SAUSSURE then considered the importance of carbon dioxide and showed that it is an essential link in the successive processes. He found, as others had found, that thin leaves deprived of carbon dioxide produced no oxygen when he placed them in an atmosphere of purified oxygen and nitrogen. In similar experiments with succulent leaves a measurable amount of gaseous oxygen was liberated although no carbon dioxide was furnished. He explained this by the fact that sufficient carbon dioxide was liberated from the fleshy leaves by respiratory processes. He said

"The exclusive property which the succulent plants have of forming carbonic acid gas from their own substance arises partly from the porosity of their epidermis or partly from contact which their interior parts have with the surrounding oxygen."

"One can have no doubt that the greatest part of the hydrogen which annual plants acquire when growing in free air with the aid of distilled water should have its origen in this liquid which they solidify. One ought to speak also of their oxygen for one cannot judge whether it came from the carbonic acid gas which these plants are able to decompose in a given time or whether from the small alteration which they cause to take place in the common air, as the quantity of oxygen which they add to the atmosphere is not at all sufficient to account for that which they acquire during the short period of their development. It must not be forgotten that water is the most abundant product of the decomposition of the majority of dry plants, or that oxygen is their principal element."

RUBEN, *et al.* in recent times showed (1941) that the oxygen liberated in photosynthesis does, indeed, come from the water molecule rather than from the carbon dioxide. The proof was obtained by using heavy water, containing O^{18} and demonstrating that the ratio of $\frac{O^{18}}{O^{16}}$ in the evolved oxygen was identical with that of the water in which the organisms grew. The authors concluded, therefore, that the evolved oxygen is derived from the water molecule.

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Section V

THE DEPHLOGISTICATED AIR OOZING OUT OF THE LEAVES IN THE WATER IS NOT EXISTING IN THE SUBSTANCE OF THE LEAVES IN THIS PURE STATE, BUT IS ONLY SECRETED OUT OF THE LEAVES WHEN IT HAS UNDERGONE A PURIFICATION, OR A KIND OF TRANSMUTATION

If the dephlogisticated air collected from the leaves in the sun existed in them in its pure state, it must appear as such when squeezed out of the leaves under water; or, at least, if the leaves are only shook gently under water, without hurting their organization, or when they are put in warm or in boiling water.

I squeezed a handful or two of potatoe leaves under water, and kept an inverted jar full of water above it, to receive the air. A great deal of it was instantly obtained, which proved to be nearly as good as common air.

I squeezed, in the same way, some air out of leaves of sage, *salvia,* which proved to be somewhat worse than the former.

A potatoe plant was shook under water, so as not to hurt it: a good deal of air was immediately disengaged, which, by the nitrous test, proved to be worse than common air.

A plant of *lamium album* was treated in the same way, and in like manner a good deal of air was obtained, which was nearly of the same quality with the former.

Some leaves of an apple tree were put in a cylindrical jar full of pump water. The jar was then inverted in a vessel full of the same water, and placed upon the fire. As soon as the water grew warm, the leaves were covered with air bubbles, just as in the sun. After the water had boiled a little while, it was put by to cool. A great deal of air was obtained, which proved to be so bad as to extinguish flame.

Some of the same leaves were put into a jar, inverted in a pot full of water, and only placed near the fire: a great deal of air was obtained, but as poisonous as the former.

Comments on Section V:— The state of the carbon dioxide in the plant cell was as unknown to INGENHOUSZ as subsequently it has been to many another student of carbon assimilation. The rapid rise in the rate of photosynthesis when a green leaf is illuminated makes it plain that a reservoir of available carbon dioxide is present and discredits any assumption that the solid carbonates stored in the cell are an important source of CO_2, yet the plastids can only obtain it from the cell sap. The view that bicarbonate ions are important in photosynthesis stems from DRAPER's discovery (1844) that plants can live in bicarbonate solutions without access to other carbon dioxide supply. He was obviously ignorant of the fact that all bicarbonate solutions contain carbon dioxide, nevertheless, the concept has enormous importance in any consideration of the photosynthetic processes of marine plants. HASSACK reported (1888)

that a green plant is able to obtain CO_2 from calcium bicarbonate, but can also make use of 70 per cent of the CO_2 in sodium bicarbonate.

The relationship between the external concentration of bicarbonate ions (for which the cell membrane is slightly permeable) and the concentration of carbon dioxide in the cell sap is not definitely known, but it has been repeatedly shown that the rate of photosynthesis may be increased by the addition of carbon dioxide to the environment of the plant. It goes without saying that not one, but many, reactions occur when carbon dioxide comes into contact with living cells. The amount chemically combined with solutes in the sap is small at low concentrations.

The absorption of carbon dioxide in excess of normal solubility, actually observed in almost all plants, must be attributed to its conversion into bicarbonates by alkalizing reagents such as alkaline earth carbonates and dissolved secondary phosphates.

Secondary phosphate absorbs carbon dioxide according to the equation,

$$CO_2 + H_2O + HPO_4^{=} \rightleftarrows HCO_3^{-} + H_2PO_4^{-}$$

SMITH found (1940) that the leaves of seven species which he tested absorbed carbon dioxide in excess of the calculated amounts, and that the absorption was reversible. He concluded that leaves contain two main carbon dioxide absorbing factors, namely, solid carbonates and a water soluble buffer. The behavior of the aqueous fraction in particular can be qualitatively accounted for by the action of phosphate buffer. The phosphate in the sap of sunflower leaves actually accounted for nearly all the bicarbonate formation. The small discrepancy indicated the presence of some minor buffering compounds.

RABINOWITCH has recently presented (1945) his ideas on the question of carbon dioxide absorption in which he postulates an acceptor whose affinity for carbon dioxide must be higher than that of the phosphate or carbonate buffers, since its saturation occurs at carbon dioxide pressures of the order of 1 mm. This value is derived from the "carbon dioxide" curves of photosynthesis (representing rate/concentration of CO_2). These curves show "half saturation" at $[CO_2]$ values of the order of 0.03 per cent in the air.

> "Thus it appears that the carbon dioxide acceptor is only loosely bound to the chloroplasts (so that it can be removed by a short contact with glucose solution) or, more probably, is not contained in the chloroplasts at all, but rather in the cytoplasm or cell sap. (RABINOWITCH, *loc. cit.*, p. 204).

RUBEN and KAMEN (1940) concluded that the first step in photosynthesis is an enzymatic carboxylation of a colorless molecule whose size and concentration is similar to that of chlorophyll. FRENKEL's experiments, furthermore, indicate (1941) that this carboxylation takes place outside the chloroplasts.

VAN NIEL has made (1941) a series of postulates which have clarified the erstwhile chaotic condition of the subject. He affirms that photosynthesis is an oxidoreduction process in which light is the activating agent and water is a hydrogen donor. Accordingly the formation of a hydrogen donor and the transfer of hydrogen are two important steps in the photochemical reaction which may be written

$$CO_2 + 2H_2O \xrightarrow{\text{light}} (CH_2O) + H_2O + O_2$$

The light-activated pigment system in the cell reduces carbonic acid, transforming the pigment system into an oxidized complex. This reaction goes on in darkness. The system cannot act successfully as a hydrogen donor unless it has been brought back to a reduced state. The oxidized pigment complex is reduced with the aid of the specific hydrogen donor (in this case, water) resulting in the re-establishment of the initial state of the pigment system with the simultaneous formation of the oxidation product, oxygen. This postulate is supported by the discovery by RUBEN, *et al.* (1941) that the oxygen evolved in photosynthesis comes from the water and not from the carbon dioxide (*vide* comments on Section IV).

VAN NIEL has also shown that a similar type of synthesis occurs in other organisms, *e.g.*, the purple and green sulfur bacteria. He concluded from a large body of evidence produced by various workers that the fundamental process in the photosynthetic bacteria in organic media is one of the general type

$$CO_2 + 2H_2A \xrightarrow{\text{light}} (CH_2O) + H_2O + 2A$$

in which H_2A, the hydrogen donor, is represented by a variety of simple organic substances. The carbon dioxide is reduced while the organic material is dehydrogenated.

This far have we come from the point at which INGENHOUSZ left the subject.

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Section VI

THE PRODUCTION OF THE DEPHLOGISTICATED AIR FROM THE LEAVES IS NOT OWING TO THE WARMTH OF THE SUN, BUT CHIEFLY, IF NOT ONLY, TO THE LIGHT

If the sun caused this air to ooze out of the leaves by rarifying the air in heating the water, it would follow that, if a leaf, warmed in the middle of the sun-shine upon the tree, was immediately placed in water drawn directly from the pump, and thus being very cold, the air bubbles would not appear till, at least, some degree of warmth was communicated to the water; but quite the contrary happens. The leaves taken from trees or plants in the midst of a warm day, and plunged immediately into cold water, are remarkably quick in forming air bubbles, and yielding the best dephlogisticated air.

If it was the warmth of the sun, and not its light, that produced this operation, it would follow, that, by warming the water near the fire about as much as it would have been in the sun, this very air would be produced; but this is far from being the case.

I placed some leaves in pump water, inverted the jar, and kept it near the fire as was required to received a moderate warmth, near as much as a similar jar, filled with leaves of the same plant, and placed in the open air, at the same time received from the sun. The result was, that the air obtained by the fire was very bad, and that obtained in the sun was dephlogisticated air.

A jar full of walnut tree leaves was placed under the shade of other plants, and near a wall, so that no rays of the sun could reach it. It stood there the whole day, so that the water in the jar had received there about the same degree of warmth as the surrounding air (the thermometer being then at 76°); the air obtained was worse than common air, whereas the air obtained from other jars kept in the sun-shine during such a little time that the water had by no means received a degree of warmth approaching that of the atmosphere, was fine dephlogisticated air.

No dephlogisticated air is obtained in a warm room, if the sun does not shine upon the jar containing the leaves.

Comments on Section VI:— Leaves placed under water produced bubbles of gas only in the daytime when illuminated by the sun. BONNET had erroneously concluded that heat was the cause of gas production (Section VII, French edition), but INGENHOUSZ differentiated clearly between the action of heat and light.

This important subject was amplified somewhat in the French and German editions. INGENHOUSZ' formulation of the problem in this brief section was clear and unmistakable for the knowledge of physics in his day, and his simple experiments gave convincing evidence that light is the prime mover of the photosynthetic process.

It may be enlightening to read the following somewhat abbreviated translation taken from the German edition. "When the sun expands the air as a result of the heating of the water, it would cause that the dephlogisticated air should develop from

the leaves immersed under water. It might then follow that if a leaf which became warmed by the noon-day sun were taken from a tree and plunged suddenly into very cold, fresh spring water, air bubbles could not be given out until the water had received at least a slight amount of heat. However, it happens exactly the opposite. The leaf which one takes from a tree or an herbaceous plant at noon on a sunny, warm day and dips immediately into cold spring water forms with especial rapidity air bubbles which consist of the purest dephlogisticated air. Were it due to the heat and not to the sunlight, one could obtain similar air if he would heat the water by means of a fire to almost the same degree of temperature that obtains in the sunshine. This is, however, generally not the case."

> "I submerged leaves in a glass vessel under water, stood the vessel inverted over spring water, and brought the apparatus so near the fire (in weak illumination) as was necessary in order to cause in the vessel a measured amount of heat of the same degree as a similar vessel with leaves of the same kind at the same time in free air received from the sun. The result was that the air liberated by the heat of the fire was very badly vitiated. On the contrary, the air obtained in the sun consisted of pure dephlogisticated air."

The experiments were varied in another way. The result was always the same. Also, in the open air, raising the temperature independent of the radiation from the sun liberated no oxygen; and, likewise, in a heated room, if the daylight was not sufficiently strong, no oxygen was evolved by the green leaves.

After many experiments undertaken with various plants and plant organs, it was always shown that only the green parts, especially the leaf blade, were capable of liberating oxygen in the light. On the contrary, all of the other non-green plant organs, either in light or in darkness, liberated vitiated air, as also did the green plants in the window.

Years passed before the relation of photosynthesis to temperature was investigated. The problem is always complicated by the simultaneous effect upon respiration. It is certain that photosynthesis may proceed at very low temperatures in some plants. Active and resistant chloroplasts are actually capable of assimilating carbon dioxide at zero degrees Centigrade (Boussingault, 1869), due, in all probability, to the lower freezing point of the cell sap. The evolution of oxygen by tropical plants may cease, according to Ewart (1898), at 4° to 8° C. whereas warm temperate, subtropical and water plants cease at 0° to 2° C., while cool temperate, alpine and arctic plants cease only when they are completely frozen.

Evergreen coniferous trees apparently are able to maintain a sufficiently high rate of photosynthesis during cold winter weather to offset the consumption of carbohydrates in respiration. Boussingault (1869) found that oxygen was produced by *Pinus laricio* and by certain grasses at temperatures of —0.5° to +3.5° C., from which he concluded that green trees and grass of the meadows may very well assimilate throughout the winter.

Miss Matthaei reported (1905) some fundamental relations from accurate determinations of photosynthetic assimilation in leaves of *Prunus laurocerasus* exposed to a series of temperatures. Photosynthesis occurred at —6° at a low rate and rose with successively higher temperatures until light was a limiting factor. At temperatures above 38° C., the photosynthetic rate fell off rapidly.

In most plants of the temperate regions, the range of temperatures within which normal photosynthetic assimilation occurs is between 10° and 35° C. because other factors become limiting outside of that range.

The temperature curve of photosynthesis differs significantly from that of respiration. At temperatures in the neighborhood of zero Centigrade, the respiration is negligibly small. The shape of the curve is conditioned by light intensity and by carbon dioxide concentration. Lundegårdh found (1925) that the absolute assimilation maximum for sun-plants growing under maximum light and normal content of atmospheric carbon dioxide is about 20° C., and an increased content of carbon dioxide (never reached in nature) raises the maximum to 30° C. In moderate shade the maximum assimilation is about 20° C., but in extreme shade the maximum is considerably lower. These temperature relations seem to have ecological significance, since, in many parts of the earth a temperature of about 20° C. is the limit of the upper range. The optimum temperature for assimilation is, therefore, a factor which places the colder regions in a relatively better situation than the warmer. As a matter of fact, the

richness and vigor of plant life in the subarctic zones is intimately connected with the position of the assimilation optimum.

The same applies to many plants of deserts and tropical regions which live where air temperatures reach 55° C. and probably assimilate carbon dioxide at temperatures only slightly below that point.

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Section VII

REFLECTIONS

It might, perhaps, be objected, that the leaves of the plants are never in a natural state when surrounded by pump water; and that thus there may, perhaps, remain some degree of doubt, whether the same operation of the leaves in their natural situation takes place.

I cannot consider the plants kept thus under water to be in a situation so contrary to their nature as to derange their usual operation. Water, even more than they want, is not hurtful to plants, if it is not applied too considerable a time. The water only cuts off the communication with the external air; and we know, that plants may live a long while without this free communication. Besides, water plants, as *persicaria urens, becabunga,* and others, which I have employed in my experiments, are often found a long while quite covered by the water in which they grow.

By bending a living plant (the root remaining in its own earth) in an inverted jar full of water, you only surprize nature upon the fact in the middle of its operation, by shutting at once all communication with the free air. In such a situation no air can be absorbed by the leaves, or by any parts of the plant under water; but any air may freely come out of it.

Without covering the leaves or the plant entirely with water, it is impossible to know what quantity of air oozes out of the plant, and of what quality this air is; for any air issuing out of a plant incorporates immediately with the surrounding air, and makes a compound whose constituent parts are an intimate mixture of air from the plant and common air; and it would be as difficult to judge accurately how much dephlogisticated air such a plant has communicated to the ordinary air which was already in the jar, as it would be for a chymist to judge accurately what quantity of distilled water was mixed with a certain quantity of common water, if some of it was really added to it on purpose to puzzle him. It may, however, be ascertained, in an inaccurate way, what quantity of this beneficial air a plant, placed in a jar full of common air, has communicated to it, by computing the degree of superior goodness the air is found to possess.

As plants yield in a few hours such a considerable quantity of dephlogisticated air, though their situation seems rather unfavourable for it when they are kept under water; may it not with some degree of probability be conjectured, that they yield much more of it when remaining in their natural situation; for then, being continually supplied by new common air, their stock of dephlogisticated air cannot be exhausted. It is an unfavourable circumstance, that air is not an object of our sight; if it was, we should perhaps see that plants have a kind of respiration as animals have; that leaves are the organs of it; that, perhaps, they have pores which absorb air, and others which throw it out by way of excretion, as are the excretory ducts of animals; that the air secreted, being dephlogisticated air, is thrown out as noxious to the plant (which article is clearly demonstrated by Dr.

PRIESTLEY and Mr. SCHEELE) ; that in the most part of plants, principally trees, the greatest part of inhaling pores are placed upon the upper side of the leaf, and the excretory ducts principally on the under side.

If these conjectures were well grounded, it would throw a great deal of new light upon the arrangement of the different parts of the globe, and the harmony between all its parts would become more conspicuous. We might find, that partial tempests and hurricanes, by shaking the air and the waters, produce some partial evils for the universal benefit of nature; that, by these powerful agitations, the septic and noxious particles of the air are blown away, and rendered of no effect, by being thus diluted with the body of air, and partly buried in the waters. We might conceive a little more of the deep designs of the Supreme Wisdom in the different arrangement of sublunary beings. The stubborn atheist would, perhaps, find reason to humiliate himself before that Almightly Being, whose existence he denies because his limited senses represent to him nothing but a confused chaos of miseries and disorders in this world.

Comments on Section VII:— The initial sentence implies that INGENHOUSZ, or someone else, had suspected that the submerged leaves might not function normally. The arguments for the results are intended to refute criticism, yet he did not seem supremely confident of their validity. In the light of many researches made subsequently, we can have confidence in his results only when the duration of the experiments was short.

INGENHOUSZ placed leaves of various plants in water and studied the gas which escaped when they were exposed to light. For him, it was the only available method for collecting the eliminated gas, as he stated above; but he overlooked the possibility that microorganisms growing on the surfaces of the submerged leaves increased the production of "fixed air." Moreover, parasites and facultative parasites which could, under the conditions of the experiment, have attacked the leaves, could have produced the congery of gases which he called "bad air." The open stomata of leaves under water would have afforded portals for the entrance of germ tubes into leaves. We are now cognizant of the large numbers of fungal and bacterial spores which rest on the surface of leaves. Under conditions which prevail in a submerged leaf culture, there are opportunities for both parasitic and saprophytic microbes to develop (YARWOOD, 1946), especially in the vascular systems of severed petioles. Their presence is often revealed by the turbidity of the water.

SENEBIER reported (1788) that detached leaves kept in darkness were overgrown with a fine network which could be removed like a spider web, whereas similar leaves kept in the light suffered no such ill effects. He concluded that the light had a possible antiseptic virtue (a conclusion which subsequently proved capable of verification).

INGENHOUSZ and others made the observation that leaves submerged in well water produced more oxygen than a similar number in river water, which probably indicates that the former contained a smaller number of contaminates. YARWOOD (1946), who discovered that younger leaves survive longer in detached cultures than older and that greenhouse-grown survive longer than field-grown leaves, concluded that the differences may be due, in part, to differences in the number of contaminates as well as to the smaller quantities of organic matter they carry.

It remained for SENEBIER to avail himself of such aquatic plants as *Caltha palustris*, Cress, *Chara*, *Myriophyllum*, *Potomageton* and the like for experiments on submerged plants and to obtain positive evidence that, when exposed to light, they produced oxygen. He felt justified in the assumption that the special organization of the leaves of aquatic plants enables them to resist the macerating action of water, and made the pertinent observation that aquatic leaves are much lower in their content of "extractive material." He failed to grasp the full significance of his results and merely concluded that the gas produced is not the product of the extractive material, thereby missing an unusual opportunity to add something that INGENHOUSZ had omitted.

The quantitative relations of assimilation ascertained by BLACKMAN and SMITH (1910) show distinct differences in the aquatic plants, *Elodea* and *Fontinalis*. The

latter assimilates about half as much as *Elodea* for any given concentration of carbon dioxide. BLACKMAN and SMITH concluded that the access of carbon dioxide to the chloroplasts was freer in the leaf of *Elodea,* noting that plants of that type have a well-developed system of air-spaces, containing an "internal atmosphere," while *Fontinalis* lacks this physiologico-anatomical characteristic. Bubble counting is beset with too many errors for quantitative work. Many varied types of apparatus for examining the gas exchanges of leaves have been devised to overcome difficulties inherent in the submerged leaf type of experiment.

BROWN and ESCOMBE (1905) set the petioles of detached leaves in water and encased the blades in flat rectangular glass chambers through which a gas stream could be drawn. The area of the leaf was determined at the close of the experiment, allowing them to compute the assimilation per square decimeter in grams per hour. The perfused gas was drawn through absorption tubes where it was analysed.

A realization of the importance of measuring the gas exchanges in leaves growing in the field or forest has impelled workers to modify for special purposes the apparatus of BROWN and ESCOMBE. A few of the techniques will be mentioned.

HEINICKE and HOFFMAN (1933*a*, 1933*b*) designed equipment with which they made a large number of determinations of the absorption of carbon dioxide of apple leaves under field conditions. The photosynthetic activity was estimated by determining the differences in carbon dioxide content between a continuous stream of normal air and a similar stream of air that had passed over leaf tissue confined in a cellophane chamber.

THOMAS and HILL (1937) constructed small glass houses and studied the assimilation of wheat and alfalfa which grew in them. The composition of the air can be automatically determined by elaborate apparatus which they designed.

CHILDERS and BRODY (1939) designed a small insulated room in which light, temperature and humidity were controlled by intricate machinery. They employed small trees growing in pots.

MEYER and RADER (1936) designed a "photosynthometer" which is almost the only recent instrument for studies with detached leaves. In the hands of an expert technician the instrument can yield results of a high degree of accuracy.

The modern reader will be deeply impressed by the certainty that the rate of assimilation in detached leaves would be increasingly retarded by the accumulation of assimilatory products. BOUSSINGAULT (1868, vol. 4) was one of the first to measure assimilation in a branch before and after removing it from the parent plant, finding that the rate of assimilation was retarded after the excision. SACHS (1884) demonstrated that starch disappears slowly from a detached leaf because there is no "sink" to which the transformed products could flow. SAPOSCHNIKOFF reported (1891) that photosynthesis in detached *Vitis* leaves ceased when the carbohydrate approximated 25 per cent of the total dry weight.

Detached leaves which are illuminated can form carbohydrates and hence live longer than those from which light is excluded. VICKERY, *et al.,* observed (1937) that excised tobacco leaves cultured in the light in water and/or glucose solution were apparently normal for 72 hours. Analyses indicated a rapid synthesis of soluble carbohydrates. Thereafter the tips and margins began to turn yellow, though the tissues along the midrib were green after ten days. Excised leaves cultured in the dark became yellow earlier; at the expiration of 143 hours they were entirely yellow and their tips were necrotic. VICKERY and collaborators found that the total solids in the illuminated leaves increased, in contrast to the steadily decreasing amounts in the non-illuminated leaves.

INGENHOUSZ made some relevant remarks in Section III concerning the activities of leaves of various species at particular periods of the day. As one might expect, they did not act identically. The differences may be referred to the diurnal changes in their content of carbohydrates, proteins and minerals. It may be assumed that leaves detached at midday or later would have more of such materials and hence would live longer than leaves detached in the early morning. YARWOOD demonstrated (1934) that clover leaflets cut from the plants in late afternoon and floated on distilled water survived longer than those cut in the morning shortly after sunrise.

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Section VIII

DRY PLANTS HAVE VERY LITTLE OR NO POWER TO AFFECT AIR; BUT, WHEN MOISTENED, THEY INFECT AIR

I filled a jar with dry hay, another with dry straw, and left it inverted a good while; but could not find the air altered.

I put some leaves of a lime tree, dried for the purpose, in a jar full of fresh pump water; and placed another jar, filled with an equal number of fresh leaves of the same tree, near it in the sun.

The dry leaves began much later than the others to yield round bubbles just in the same way, but which did not grow so quick, nor so large. However, in a few hours, a good deal of air was obtained, but of such a bad quality as to extinguish a flame, whereas the fresh leaves had yielded dephlogisticated air: which experiment seems to indicate, that the generation of the dephlogisticated air is owing to the action of the living plant. The same result was also obtained from dry hay put into a jar full of pump water.

Section IX

ALL PLANTS POSSESS A POWER OF CORRECTING, IN A FEW HOURS, FOUL AIR UNFIT FOR RESPIRATION; BUT ONLY IN CLEAR DAY LIGHT, OR IN THE SUNSHINE

This remarkable property of plants is indeed very great; for in a few hours, nay even sometimes in an hour and a half, they purify so much a body of air quite unfit for respiration, as to be equal in goodness to atmospheric air. They will even do it when they are inclosed in a glass vessel, without any water. One leaf of a vine, shut up in an ounce phial, full of air fouled by breathing so that a candle could not burn in it, restored this air to the goodness of common air in the space of an hour and a half. But plants enjoy this privilege only in the day-time, and when they grow in unshaded places.

This power of plants extends itself even to the worst of all airs, in which an animal finds his destruction in a moment; such as is pure inflamable and highly phlogisticated air, which is little or scarcely at all diminishable by nitrous air. I observed some difference in various kinds of plants in this respect, and found that water plants seem to possess this quality in a greater degree than others. The willow tree and the *persicaria urens* were found eminent in producing this effect: and may it not be providentially ordained it should be so, as those plants grow better in marshy, low grounds, and even in stagnated waters, whose bottoms are generally muddy, and yield a great deal of inflammable air, which may be collected at the surface of the water by stirring up the ground, and may be kindled by throwing a burning paper upon the water, which is an amusing experiment by night? Plants, however, want longer time to correct this kind of air, at least that which is extracted from metals by vitriolic acid.

The property of plants is demonstrated in experiments 41, 51, 56, 57, 58, 59.

Comment on Section IX:— To supplement this section, the following record taken from p. 263 of the French edition may be added.

In the afternoon Ingenhousz filled a jar with air which had been so vitiated by respiration that it extinguished a candle flame. He placed in it a sprig of the common nettle and kept the jar in the house overnight. The next morning the air in it was as bad as when he had placed the nettle into it. After he had removed some air necessary for making the test he placed the jar in the sunlight at nine o'clock. This same plant which had not improved the air during the night, so corrected it in two hours in the sun that it was practically as good as the common air.

Section X

ALL PLANTS YIELD A MORE OR LESS QUANTITY OF DEPHLOGISTICATED AIR IN THE DAY-TIME, WHEN GROWING IN THE OPEN AIR, AND FREE FROM DARK SHADE

The quantity of dephlogisticated air, and even the quality of it, which the leaves of plants give, seems to be different in different plants: though, indeed, this may depend in a great measure upon some particular circumstances, to which it is not easy to be sufficiently attentive. It seems, however, to be a general rule, that the leaves of all plants, growing in a place where they are not much shaded by other plants, buildings, &c. yield, in a clear day, dephlogisticated air; and that this air is yielded in greater abundance, and of a greater purity, when they grow in open places unincumbered by other plants higher than they are themselves.

I got in general a large quantity of air of a very good quality from some water plants, as from the *persicaria urens* and willow. The fir trees yielded also very fine air, and in abundance.

The *nasturtium Indicum* surpassed them all in general, in regard as well to the quantity as to the quality. One hundred leaves of this plant, which are very thin, yielded, in two hours time, as much dephlogisticated air as would fill a cylindrical glass four inches and a half deep, and one inch and three quarters diameter; of which quantity gathered again afterwards from the same leaves, without taking them out of the water, see exp. 30-35. This quantity surpasses by far the bulk of the leaves themselves, and shews to how amazing a quantity the air may amount yielded in a fair day by a lofty tree.

The leaves being more or less crowded together, being exposed for a longer or shorter time, or sooner or later in the day, will occasion some difference in the quality and quantity of this air.

It seems that, in general, the finest air is obtained when the sun has passed the meridian.

Section XI

THE FACULTY WHICH PLANTS POSSESS OF YIELDING DEPHLOGISTICATED AIR, OF CORRECTING FOUL AIR, AND IMPROVING ORDINARY AIR, IS NOT OWING TO THE ACT OF VEGETATION

If this wonderful faculty of plants depended upon their vegetation, they would exert it at all times, and in all places in which the vegetation goes on. A plant may vegetate, and even thrive very well, in the utmost darkness; and yet in such a place it has no power to correct bad air, or to yield good; but, on the contrary, it spreads round about it deleterious exhalations, which render the best air even pernicious to the utmost degree.

It will not be difficult to understand now from what cause all those different and contrary effects which Dr. PRIESTLEY has found in his experiments did really depend; and why Mr. SCHEELE had constantly found that the vegetation of beans always spoilt good air.

These gentlemen expected the good effects from the vegetation of the plants, as such. By making a plant grow night and day in ordinary air kept in a phial with the plant, the effect will depend upon the greater or less exposure of the plant to the light. Besides, by keeping a plant a long while in pump water, the green matter, of which Dr. PRIESTLEY found to issue very fine dephlogisticated air, will be generated; and thus the air within the phial, being mixed with this good air, will not in reality indicate the effect of the plant upon this air, as Dr. PRIESTLEY makes no scruple to acknowledge in his late work, p. 338.

Comment on Section XI:— INGENHOUSZ in this brief section corrected an error which, as previously stated, PRIESTLEY and others had made, but he presented no experimental data. He arrived at the correct conclusion by a process of rationalization. INGENHOUSZ had previously said that "one could also determine easily why Mr. SCHEELE came to the conclusion that the air in the vessels was vitiated by the beans growing in them. These men unfortunately expected the afore-mentioned wholesome effect from the growth powers of the plant. If one lets a plant stand day and night in a vessel filled with ordinary air, the outcome of the experiment will depend upon the degree of illumination the plant receives." Neither SENEBIER nor DE SAUSSURE seem to have been interested in the question, though the latter demonstrated that oxygen is necessary for growth. When it was discovered that the chloroplast is the site of photosynthetic activity and that etiolated plants in darkness can grow, people ceased to be interested in the subject. A recent contribution, however, presents some actual determinations. RICHTER, SUKHORUKOV and OSTAPENKO (1944) studied the relations between processes of growth, production of substance and photosynthesis in *Beta vulgaris* and found that the rate of carbon dioxide decomposition per unit area was about the same in plants differing widely in the development of their organs and in rate of growth. They concluded that there was no functional relationship between photosynthesis and growth.

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Section XII

THE PLANTS EVAPORATE BY NIGHT BAD AIR, AND FOUL THE COMMON AIR WHICH SURROUNDS THEM; YET THIS IS FAR OVERBALANCED BY THEIR BENEFICIAL OPERATION IN THE DAY

The bad air which plants yield by night is so inconsiderable in comparison of the quantity of dephlogisticated air which they yield by the daytime, that it amounts to very little. By a rough calculation, I found that the poisonous air yielded during the whole night by any plant could not amount to one-hundredth part of the dephlogisticated air which the same plant yielded in two hours time in a fair day. But, from my experiments, one might naturally wonder that no remarkable mischief happens from so many plants as a forest contains, if one plant, containing scarce a handful, may poison to such a degree the quantity of two pints of common air in one night, as to render it absolutely mortal for any animal who breathes in it.

I make no doubt but a great quantity of plants, kept in a close and small place during a night, or by day in the dark, may do some material mischief, and even occasion death, to any person who should be imprudent enough to remain in such a place. The undoubted facts of people being found dead in their beds, when they had slept in a small room with a great deal of flowers in it, must inspire us with a caution against keeping too many flowers in close rooms. My experiments go much further, and will, I hope, in future, make people aware of danger if they store up a great deal of fruit in a close room in which they sleep.

I think that nobody before me even suspected the least danger of keeping beans, peaches, or such like fruits, in their rooms; and yet a sufficient number of them would easily poison an unwary person sleeping in a close room, in which an abundance of these fruits is stored.

The gardeners by opening a hot-house early in the morning, which has been shut close during the night, or at any time in the day if the sun has not shined a good deal upon it, are very well aware of a particular oppression they feel by entering it. I remember to have felt it more than once, without even suspecting the cause of it. Dr. PRIESTLEY observed this remarkable offensiveness of the hot-houses with a more philosophical attention; he tried the air within them, and found it worse than common air.

By all this it seems evident, that it would not be safe to sleep in a close hot-house; that it would not be prudent to keep too many green branches, fruit, or flowers, in any room by night, particularly in that of a sick person.

The best physicians have, indeed, often recommended to put green branches of lime-trees and others in the rooms of their patients, without ever suspecting any other effect but benefit from them. I think still, that

some benefit may arise from putting, in a clear day, fresh green branches in the room of a sick person, by cooling the air, which is owing to the evaporation of moisture; but I should now apprehend rather some mischief from them in a room whose doors and windows are shut, and which is not well lighted. At any rate, I should no more allow them to be kept in the night-time in the room of any of my own patients.

Is it not somewhat probable, that among those people who are found dead in their bed without any previous illness, some may owe their untimely end to some such concealed cause, which nobody ever suspected to be in any way dangerous?

But the mischief which trees in reality do by night-time to the surrounding air, cannot do any observeable harm to animals: for those mischievous exhalations being, very providentially, specifically lighter than common air, rise at the same time up; and thus the lower region, in which we breathe, is freed from them almost as soon as they are produced; whereas the dephlogisticated air issuing out of the plants in great abundance in the day-time is specifically heavier than common air, and is therefore inclined to remain longer among us, and to afford us all the benefit for which the Supreme Wisdom has providentially destined it.

Comments on Section XII:— The theory that health is endangered by the presence of plants in houses or by trees in the immediate vicinity was a pretty wild guess, on the part of INGENHOUSZ, and, so far as known, he never subjected it to an experimental test. It is another evidence that INGENHOUSZ was first and last a physician.

SENEBIER was quick to contradict the idea, thereby initiating a bitter controversy in which INGENHOUSZ never admitted his error.

Although we may smile at INGENHOUSZ' bungling theory, we must admit that he was groping for a rational idea of the processes which, in modern parlance, we call photosynthesis and respiration. Eventually, he expressed some fairly correct ideas on the interrelations between the two processes, but it remained for men like DE SAUSSURE (1804) to make quantitative studies on them. Subsequently, the phenomenon was further investigated and new studies are being continually made.

The so-called assimilatory coefficient, $\frac{O_2}{CO_2}$, the ratio between the oxygen evolved and the carbon dioxide absorbed, is approximately unity. DE SAUSSURE discovered, however, that it may often lie below that value, because the observed evolution of oxygen is less than that actually formed in photosynthesis by the amount used in respiration. At the same time, the observed absorption of carbon dioxide is less than that utilized, since some of the gas is evolved in respiration. Some of DE SAUSSURE's observations on this subject have been included in my comments on section IV. STILES has given (1925) an excellent account of investigations on this relationship which seems to have baffled INGENHOUSZ.

The initial stages of photosynthesis in INGENHOUSZ' submerged-leaf experiments, must have been dependent on the small amount of oxygen dissolved in the water. His observations on the failure of leaves to produce dephlogisticated air when submerged in water previously boiled seem pertinent to this idea. The necessity of a low partial pressure of oxygen for photosynthesis was demonstrated by BOUSSINGAULT (1864) and the inhibiting power of high partial pressures has been reported (1928) by WARBURG. This is logical if we grant that the first step is essentially a photo-oxidation, which, if it increases, may block the mechanism which accepts hydrogen from a suitable donor (*vide* FRANCK and GAFFRON, 1941). It seems the more probable that the photo-assimilatory mechanism is in some way dependent upon the respiratory process. GAFFRON (1939) drew attention to the fact that the photosynthetic apparatus, working as an oxido-reduction system, is influenced by molecular oxygen in a way which resembles the effect of oxygen on fermentation known as the "Pasteur reaction."

The highest efficiency of photosynthesis is found at rather low light intensities, *i.e.*, where respiration undoubtedly surpasses assimilation. At high light intensities, assimilation so far exceeds respiration that the latter is difficult to estimate.

The dynamics of photosynthesis have been a knotty problem, and many of the assumptions previously made proved to be unverifiable when experimentally tested. Photosynthesis is interrelated with other processes of metabolism, in which proteins and fats are synthesized and, *per contra,* many cell components are being continuously broken down. Always the process of respiration proceeds in the same cells, and presumably in the chloroplasts themselves.

Theories that the conversion of inorganic to organic phosphate should be greater in light than in darkness (RUBEN, 1943; LIPMANN and TUTTLE, 1945), postulate that the reducing power of photosynthesis is utilized, not only for the reduction of carbon dioxide, but also for the formation of high-energy phosphate bonds. EMERSON, *et al.* (1944) have also postulated that it may result directly in the production of high-energy phosphate. It is possible that phosphate turn-over is in some way involved (*See* comment on Section V, p. 336). ARONOFF and CALVIN (1948) failed to find however, supporting evidence from their experiments with chloroplasts and leaves in cultures containing a radio-active isotope of phosphorus. Yet, they recognized the possibility that light may accomplish transformations of organic phosphorus from one form to another without passing through the inorganic state.

It has not been demonstrated whether the intermediates of photosynthesis serve as substrates of respiration, but it is certain that the end products are highly important. Cells which contain no chloroplasts are the sites of active respiration. Since the photosynthetic processes in green cells may be inhibited by hydroxylamine without an apparent change in respiration, GAFFRON (1939) concluded that respiration in green cells is not directly dependent upon the intermediates of photosynthesis.

There are possibly two series of phenomena in the oxidative processes in living cells in the presence of an oxidizable substrate. PLANTEFOL, who measured the intensity of respiration of *Hypnum triquetrum* immersed in varying concentrations of sodium nitrate and of glucose, showed the contrast between them. He distinguished (1937) (1) the normal biological oxidations which are inherent in the functions of living matter, even in the absence of a substrate, if oxygen is present: and (2) oxidations occurring at the surface of the living cell, and depending, not on the vital functions characteristic of life, but on the contact of oxygen with an oxidizable substrate at the surface of the living cell. He designated these processes as intrinsic and as extrinsic oxidations.

These fragmentary remarks are primarily intended to emphasize the importance of the problem which INGENHOUSZ opened in this section of his book, and to indicate that physiologists are still grappling with it.

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Section XIII

ALL ROOTS, FEW EXCEPTED, WHEN LEFT OUT OF THE GROUND, YIELD BY DAY AND BY NIGHT FOUL AIR, AND INFECT THE SURROUNDING AIR.

The experiments I made upon this subject convinced me that roots have this deleterious power, and some even to so great a degree, that it would not be safe to remain in a small close room in which a great quantity of roots of plants are kept. The roots of some aquatic plants are remarkable apt to foul the air in which they are placed, such as roots of rushes, though ever so well cleaned from mud and dirt, and the roots of *persicaria urens*. But I found the roots of *becabunga* almost inoffensive in this respect, which I was the less surprized at, as their substance is but little different from the stalks. The roots of a mustard plant gave in the sun-shine a good deal of air, when kept under water; which air was worse than common air, and extinguished a candle. These roots even corrupted good air in the middle of the sun-shine.

A plant, with roots and all, placed in a jar full of water, did, however, yield dephlogisticated air; so that the bad effect of the roots was overbalanced by the plant itself.

Section XIV

FLOWERS OOZE OUT BY DAY AND NIGHT AN UNWHOLESOME AIR, AND SPOIL, AT ANY TIME AND IN EVERY PLACE, A CONSIDERABLE BODY OF AIR IN WHICH THEY ARE PLACED

Dr. PRIESTLEY has observed, that a rose, kept in a glass, had, in a short time, so much infected the air as to be unfit for respiration, and concluded from this very justly, that flowers might be hurtful in a room. I have heard more than once of a person found dead in a room where a great deal of flowers were kept; and I make no doubt but some of these cases are well founded.

I tried all the flowers I could find in my neighbourhood, but could not discover one which did not yield poisonous air, though in a small quantity, by day and by night, and which had not the power of rendering quite unfit for respiration a very considerable body of common air. They even seem not to lose in the least their deleterious influence in the sun; so that I cannot but think that it is unsafe to keep in a close room a large quantity of any flowers, even such as have the most delightful smell. I am, however, very far from thinking that there is any danger to apprehend from such nosegays as are commonly kept in a room, either for ornament or perfume. The malignant influence which could be expected from such a small quantity of flowers is entirely dissipated in the mass of the surrounding air; but the excess must not go too far, if the room is closely shut and but small. If a few flowers of the honey suckle (*caprifolium*), which possess the most agreeable smell, are able to foul in three hours time, in the middle of the day, a body of air equal to two pints (see experiments 65-70) we may judge what dangerous effect might be expected from a large quantity in a close room. Those flowers, like all others, after having rendered truly fatal a body of air, have lost nothing of their flavour. The air itself, which they have poisoned, is impregnated with the same fragrant smell as the flowers themselves; so that a person, not aware of the concealed poison which flowers spread round about them, might be easily induced by the sweetness of their scent to run the greatest hazard of losing his life, without the smallest apprehension of danger.

Section XV

ALL FRUITS IN GENERAL EXHALE A DELETERIOUS AIR BY DAY AND BY NIGHT, IN THE LIGHT AND IN THE DARK, AND POSSESS A REMARKABLE POWER OF SPREADING A POISONOUS QUALITY THROUGH THE SURROUNDING AIR

I was, indeed, not a little surprized to find this effect in even the most delicious fruit, such as peaches, grapes, apples, and mulberries. By what I observed in my experiments I am apt to think, that the power of fruit, at least of some, surpasses the deleterious quality of flowers in the dark; but the influence of the sun seems to check, in some degree, this hurtful quality in some fruit more than roots and flowers, of which the most part preserve, even in the brightest sunshine, their virulent effects upon the ambient air in its full force. I found, that one peach was able to render entirely poisonous, in a few hours, a body of air at least six times greater than the space it occupied; and even that they could, in the middle of the sun-shine, render such a quantity of air so unwholesome, that a candle could not burn, nor an animal breathe in it.

After I had observed, that all leaves of plants yield dephlogisticated air by day-light; and considering, that in general all leaves are green, and that that substance which Dr. Priestley discovered to yield so much dephlogisticated air is of the same colour; I had some suspicion, that green fruits, such as beans, would also yield dephlogisticated air. I placed, for this purpose, some French beans in a jar full of pump water, and exposed it to a bright sun-shine during four hours, and obtained a moderate quantity of air, oozing out of their substance by bubbles, in the same manner as out of leaves. This air was far from being dephlogisticated air; for it was even worse than common air, though it approached pretty near it in goodness.

I then wanted to see what effect these green fruits would have upon a body of air in the dark; and I was not a little surprized to find, that they had a very remarkable power to foul a great body of air, in which they were shut up, to such a degree, that two dozen of small French beans, placed in a jar holding two pints, had rendered in one night the air in the jar absolutely poisonous, so that a young chicken placed in it was killed in less than twenty seconds. I found even this deleterious influence of beans upon air to surpass the power of plants, which are known to be of a poisonous quality. See experiments 75-91, and principally experiments 88 and 89.

Ripe mulberries, filling one-third of a jar, and placed in the sun during four hours, had so much fouled the air within the jar that a candle would not burn in it.

Comments on Section XV:—The respiration of fruits, incidentally discovered by Ingenhousz, has in modern times received extensive study because of its scientific

and technical implications. The gases evolved by fruits constitute important problems in the ripening, storage and transport of fruits, either with or without refrigeration.

Respiration is generally regarded as an oxidation process because the final products, CO_2, H_2O and kinetic energy, are the same as those derived from the combustion of organic substances in the air. It is a rather involved process of breaking down respirable materials, through numerous stages, to more or less oxidized final products and is catalyzed by various enzymes whose characteristics are, as yet, imperfectly ascertained. The nature of the substrates on which they work has been only partially determined, but we know that sugars, proteins and organic acids are among the most important.

The following paragraphs contain brief accounts of a few recent studies upon the respiration of fruits, which, far from being complete, may serve as examples of researches in the field which INGENHOUSZ opened.

The respiratory rate of apples is very high soon after fruit setting and declines during the growth of the fruit. It subsequently rises to a secondary maximum at about the time that most apple varieties are harvested. The oxidase activity of apples in Wenatchee, Washington, decreased during the growing season (EZELL and GERHARDT, 1942), and the catalase activity increased throughout the period from July to November. It is not certain, however, that there is any direct correspondence between oxidase and catalase activity in the apple, and respiration.

EZELL and GERHARDT found (1938) that there was a positive correlation between the rate of respiration and the oxidase and catalase activity in Bartlett pear fruits on the tree, from the time they were very small until they reached commercial harvest maturity. If allowed to remain longer, the oxidase activity continued to decline, though the rates of respiration and catalase activity increased. So long as the fruits remained on the tree, there was a positive correlation between catalase activity and the rate of respiration.

The rapid decline of respiration during the first month of growth of apples corresponds, according to KROTKOV (1941), to a period of rapid cell multiplication, during which there is a heavy consumption of sugars for the synthesis of new proteins. When, in the subsequent stages of cell enlargement, this demand subsides, the excess of sugars is converted into starch.

LEONARD and WARDLAW found (1941) that the carbon dioxide content of banana fruits during storage and ripening was characterized by steady values during the unripe stage, rising to a maximum at the ripe stage, then declining. An increase in the oxygen content of the surrounding air accelerated their ripening, whereas a decrease of concentration to 10 per cent or less produced a delay.

The respiration of tomato fruits gradually increased during their growth on the plant, but decreased during and after ripening (CLENDENNING, 1942). Although exhibiting the usual senescent drift as they changed color, fruits grown in summer in weak light under cloth showed a lower rate of respiration than that observed in all other populations.

It is not hard to realize that the processes of metabolism in fruits of different plants should be dissimilar, depending on their structure, on their chemical composition and water content. Neither should we forget that cells in the interior may not be able to obtain sufficient oxygen for complete aerobic respiration. Hence it is easy to understand that no general statements can be made on the principles which govern respiration in fruits.

During the development of the apple on the tree, there seems to be more or less of an inverse relationship between sugar content and respiration, but immediately after it is harvested the losses of sugar are greater than the chemist can account for by respiration. ARCHBOLD and BARTER observed (1934) an excess of 17 to 30 per cent of the amounts of carbon lost, over and above the amounts lost as carbon dioxide in Bramley's Seedlings. Be it noted, however, that they utilized quartered fruits in their study. KROTKOV and HELSON, who used entire fruits of the McIntosh apple, confirmed (1946) earlier results, showing that after harvest the amounts of sugars disappearing from the apples were measurably greater than the amounts attributed to the escape of carbon dioxide.

HUMPHREY and DUFRÉNOY have rendered important service (1944) to the physiologist who is concerned with respiration by formulating the concept of non-compensated respiration in cells whose vitality is reduced by any cause which disturbs their organization. Non-compensated respiration means that oxygen absorption is increased

without carbon dioxide emission being correspondingly increased. Part of the oxygen may be used to dehydrogenate phenolic compounds to quinones which, in themselves, exert an inhibitory effect on some intermediate link in the respiratory system of the living cells.

BLACKMAN proposed (1928) a scheme to account for the process of respiration in apples, which may be sketched as follows:

1) Hydrolysis of starch and disaccharoses to hexoses
2) Activation: formation of heterohexoses with the less stable type of internal ring structure (isomeric change of the hexose molecule)
3) Glycolysis: involving the action of the zymase complex, includes the degradation of activated hexose to methyl glyoxal, pyruvic acid, acetaldehyde, and probably lactic acid.
4) Aerobic respiration: concerned with the production of CO_2 and H_2O from the intermediate substances produced in glycolysis
5) Oxidative anabolism.

The same author noted that the respiration rate of apples decreased with diminishing oxygen supply until a minimum concentration of five to ten per cent of this gas was reached. As the concentration of oxygen was increased above this point, the rate of respiration continued to increase until this gas composed approximately one hundred per cent of the gaseous medium.

BLACKMAN concluded that oxygen may have a dual role in respiration. One of these is the direct oxidative effect of oxygen upon a carbon substrate, while the other is the role of an activator of the carbohydrate metabolism which determines the production rate of the specific substrate which is oxidized. His affirmation that glycolysis is the common measure of respiration in all conditions is not supported by subsequent work, which shows that there is non-compensated respiration in ripening fruits. No crucial evidence has yet been presented to show whether the substances which are oxidized in the fruit cells undergo chemical changes involving the loss of hydrogen, concomitantly with the building up or breaking down of energy-rich phosphorus linkages.

Although the respiration of potato tubers in storage has been studied extensively, the mechanisms involved are by no means discovered. The respiratory rate also shows two phases, first a rapid rise, followed by a decline to a somewhat constant level, and second a smaller rise later in the season. It seems probable, from the work of BOSWELL and WHITING (1938), that polyphenol oxidase controls the major part of the respiration of sliced potato tissue. Since the addition of ascorbic acid was responsible for a temporary rise in oxygen intake and a rise in carbon dioxide output, followed by a depression, it was suggested that it might be the co-enzyme of a redox system not directly involving oxygen. BOSWELL believed that the processes which are not controlled by polyphenol oxidase are associated with dehydrase actions in which succinic, fumaric, and malic acids are involved.

BARKER made (1936) a careful study of the relation between sugars and respiration in potatoes, finding strong evidence that sucrose is closely related to the substrate supply for respiration, whereas neither glucose or fructose is directly associated with the function. He postulated a balancing mechanism between sucrose and the hexoses which is markedly affected by certain temperature changes.

Scientific, as well as historic interest, therefore, is connected with the early discovery by INGENHOUSZ that fruits respire measurable quantities of "deleterious air" either in the darkness or light. The surprise which he felt impelled him to test the gases evolved by many sorts of fruits, and he was cognizant of the difference between green and non-green fruits. Extensive studies on the phenomenon were not undertaken until recently, and further work must be done to clarify the subject.

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Section XVI

THE POWER OF PLANTS IN CORRECTING BAD AIR IS GREATER THAN THEIR FACULTY OF IMPROVING GOOD AIR

The experiments already known of Dr. PRIESTLEY, by which it appears that plants thrive wonderfully well in air fouled by breathing and burning of candles, gave me a great suspicion, that the power of plants in correcting bad air might surpass their faculty of improving good common air. In order to put my conjecture to the trial, I placed at eleven o'clock, in a warm sun-shine, two jars of an equal size, each containing an equal quantity of sprigs of pepper-mint, in pump water. In one of these jars was let up a certain quantity of common air, whose test was at that time such, that one measure of it with one of nitrous air occupied 1.06½. In the other jar was let up the same quantity of air fouled by respiration, of which one measure with one measure of nitrous air occupied 1.34.

The air of both jars was examined at two o'clock, when I found the common air so much improved, that one measure of it with one of nitrous air occupied 1.00. The foul air was so much mended, that it was near as good as the atmospheric air, for one measure of it with one of nitrous air occupied now 1.08½.

I examined both airs again at four o'clock, when the common air was still more improved; for one measure of it with one of nitrous air occupied 0.95. The foul air now was not only become as good as respirable air, or air of the atmosphere, but even surpassed it in goodness, for one measure of it with one of nitrous air occupied 1.05.

Now, as the same plant brought the foul air from 1.34 to 1.05, and the common air from 1.06½ to 0.95, it appears clear, that the plant had corrected the foul air far more than it had improved the common air.

This experiment was repeated several times with nearly the same results.

As plants seem to delight in foul air, probably because this air impregnated with phlogiston affords more proper nourishment, *viz.*, phlogiston to the plant; it must of course happen, that a plant draws to it so much the more phlogiston as the air, in which it grows, contains more of this principle.

When a plant grows in the open air, it contaminates by night the surrounding air; but this air, being diluted with other air, does not appear in reality to be altered by any method yet found out: besides, it is probable, that this air is risen up as soon as it was become phlogisticated by the plant, being specifically lighter than common air. It seems therefore not improbable, that some plants, as for instance the *hyoscyamus,* may contaminate in reality more air at night than they improve in the day; so that, if all the air spoiled by such a plant was shut up with the plant a whole night and a day, the air would still be found contaminated: but tho' this might

be the case when the plant is shut up with the air, yet it could never be any real disadvantage in the natural situation of things, because this foul air may be corrected in the atmosphere by some manner or other unknown to us; and, if not, we are, at any rate, immediately out of its reach, as it rises by its being become lighter. But if such infectious plants are shut up in small close rooms, they certainly might do a material injury to our constitution, and even occasion death.

It appears, by experiment 41, that a plant may really foul so much air at night as scarce to be able to correct in the day. But it is to be considered, that such a plant, being maimed by its roots being taken off, and by being shut up in a narrow space, must have lost some of that vigour which plants naturally have when they remain undisturbed upon their place. See also experiments 51, 56, 57, 58, 59, 60.

Comments on Section XVI and related subjects have been given after Section XII.

Section XVII

ON THE EFFECT OF LEAVING PLANTS KEPT IN A ROOM

Though I think, that the keeping of a few plants in rooms is very indifferent as to the health of the persons who live in them; yet it is not so indifferent for us to know the effects which plants have in reality on the air of the room, that we may avoid danger from any excess.

The influence of plants on the air of a room in which they are kept is different in the night from what it is in the day. In the day plants are apt to contribute somewhat to purify the air of the room, if they are placed so as to receive all the light of the sun possible: if they are placed so as not to receive the direct influence of the sun, but to be free from any shade, they seem to have no influence at all, either in improving the air of the room or in fouling it. But when they are placed in a part of the room the most remote from the windows, so as to be much shaded, they are apt to render the air of the room more or less impure, in proportion to their size, and to the more or less degree of light of the place where they stand. At night they absolutely tend to foul the air, principally when they flower. I acknowledge readily, that a few flower-pots can do neither good nor harm. But I remember to have found several orange trees in a room, by way of ornament, and, as I was told, to keep the air of the room wholesome: I think now such ornamental plants by no means indifferent, unless they were but small and the room ample; at any rate I should not suffer them to be kept in a room at night, where a sick person is.

A plant shut up in a glass jar, and placed near the window in a room so as to receive the rays of the sun, will make the air of the jar better than the air of the room: whereas a similar plant, placed in the same room in a shaded place, will render the air of the jar worse than the air of the room. If, after a few hours, you invert the experiment, by placing the plant which stood at the window in the shade, and that which stood in the shade near the window, the reverse will take place, *viz.*, the air of the jar, which was improved, will be found worse than the air of the room; and the air of the jar, which had been contaminated, will be found corrected again. And this remarkable property of plants, in the way just mentioned, may be demonstrated in a few hours. See experiment 45.

Section XVIII

LEAVES OF PLANTS DIE SOONER WHEN THE DEPHLOGISTICATED AIR, ELABORATED BY THEM, IS SEPARATED FROM THEM

When the dephlogisticated air, settled in the form of bubbles upon the leaves, is shook off, new bubbles succeed; and thus by shaking off several times these air bubbles a greater quantity of dephlogisticated air is obtained. The second crop of bubbles contains in general a finer kind of air than the first; the reason of which may be, that it is scarce possible to free the surface of the leaves entirely from all atmospheric air sticking to them, particularly those which have a rough shaggy surface; as, for instance, the leaves of sage, *salvia.*

Some of these leaves are so prolific in pushing out these bubbles, that I have found them reproduced nine or ten times in leaves of a pear-tree. The leaves of a vine are also very ready to yield a good number of successions of these bubbles. But I was curious to see whether the leaves decay sooner or later when the air bubbles were left upon them, or when they were shook off now and then: I put a leaf of a vine in a jar full of pump-water, and left it exposed to the open air without ever stirring it: the air bubbles grew to a very large size; and some of them quitted the leaf of themselves and rose up. This leaf remained as fresh as when it was put in the jar during a whole week; whereas another leaf of the same vine, placed near it in another jar, and whose air bubbles were shook off five or six times in a day, was withered in less than two days. This second leaf had lost the greatest part of the rough surface, which covers, as a kind of scarf-skin, the under and unvarnished part of the leaf; at least this scarf-skin became transparent, if it was not really destroyed; and this transparency was observed principally upon the very spots of the air bubbles. This experiment was repeated several times with the same success.

It should seem by this observation, that the loss of this air, if it cannot be replaced by the absorption of new air from the atmosphere, makes the leaves decay sooner; and thus the texture of the leaves, having no more air to elaborate, resembles almost the organization of an animal, which loses its life by becoming exhausted through the losses sustained by the increase of the various excretions which are carried on in its body, if these losses are not repaired by taking in new nourishment.

Vegetables seem to draw the most part of their juices from the earth, by their spreading roots; and their phlogistic matter chiefly from the atmosphere, from which they absorb the air as it exists. They elaborate this air in the substance of their leaves, separating from it what is wanted for their own nourishment, *viz.*, the phlogiston, and throwing out the remainder, thus deprived of its inflammable principle, as an excrementitious fluid, and in this state hurtful to them, but rendered useful to the animals,

who in their tour take from this air, by the act of respiration, what they want, and throw out the remainder as hurtful to them; but rendered again serviceable to the vegetables. This theory seems to be very reasonable, and to have some foundation in nature. It throws a good deal of light upon the economy of nature, and the mutual influence which the vegetable kingdom has upon the animal, and the animal upon the vegetable. It has some analogy with other general operations of nature, which are well known.

A plant, which is a living being, destitute of motion, remaining upon the same spot on which it took its beginning, is not capable, as animals are, of going in search of its food, must find within the narrow compass of the space it occupies everything which is wanted for itself, and to fulfil the office which has been dictated to it by the Author of nature. It is obliged to spread the numberless filaments of its root through the surrounding ground, as so many siphons to pump up the juice, which presents itself to those filaments; and these filaments are sufficient to afford all that the greatest part of trees want in the winter. But, being destined in the summer-time to more important offices, the tree spreads through the air those numberless fans, disposing them, in the most advantageous manner imaginable, to incumber each other as little as possible in pumping from the surrounding air all that they can absorb from it, and to present, if I may so speak, this substance drawn from the common atmosphere to the direct rays of the sun, on purpose to receive the benefit which the influence of that great luminary can give it.

Comment on Section XVII:— The importance of a supply of oxygen to leaves was skillfully shown by the experiments described in this section. While he recognized the evolution of oxygen, INGENHOUSZ did not envisage the possibility that plants simultaneously evolve carbon dioxide by respiration, though he was clear enough about the phenomenon when light was excluded.

BLACKMAN and SMITH (1911) made a critical study of gaseous exchanges in plants which answered the question that INGENHOUSZ could not solve. They measured the amounts of carbon dioxide formed in respiration when light was excluded and at four different temperatures. They then passed a current of water, in which definite amounts of carbon dioxide had been dissolved, through the glass chamber containing *Elodea* plants and determined the amounts of the gas in the effluent stream with correction for the carbon dioxide which escaped in the form of bubbles. This was the apparent assimilation. The real assimilation was determined by adding the amount of carbon dioxide (calculated from previous measurements) for respiration at 20.3° C. The amount of carbon dioxide formed by respiration during the experiment was approximately 9 per cent of the real assimilation.

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Section XIX

ON THE POWER WHICH VEGETABLES HAVE OF ABSORBING DIFFERENT KINDS OF AIR

If a plant is shut up in a certain quantity of air, and all light hindered from falling upon it, it absorbs in general more air than it yields, and therefore the bulk of air is found less. The quantity of air thus absorbed by plants may vary from numberless circumstances, as well as from the particular nature of the plant. I have no time to search in my notes for all the particularities I have observed upon this subject. I can say in general from remembrance, that some water-plants were very willing to absorb a good deal of air, principally when they were placed with roots and all in the air; and that they readily absorbed air fouled by breathing.

One of those plants had also absorbed a great deal of dephlogisticated air, so that in one night it had absorbed half of the quantity I had put with it, which amounted to 4 ounce measures, if I rightly remember.

This absorption also takes place in the day time; but as the plants at that time yield themselves a great quantity of air, the absorption is not so easily ascertained.

Section XX

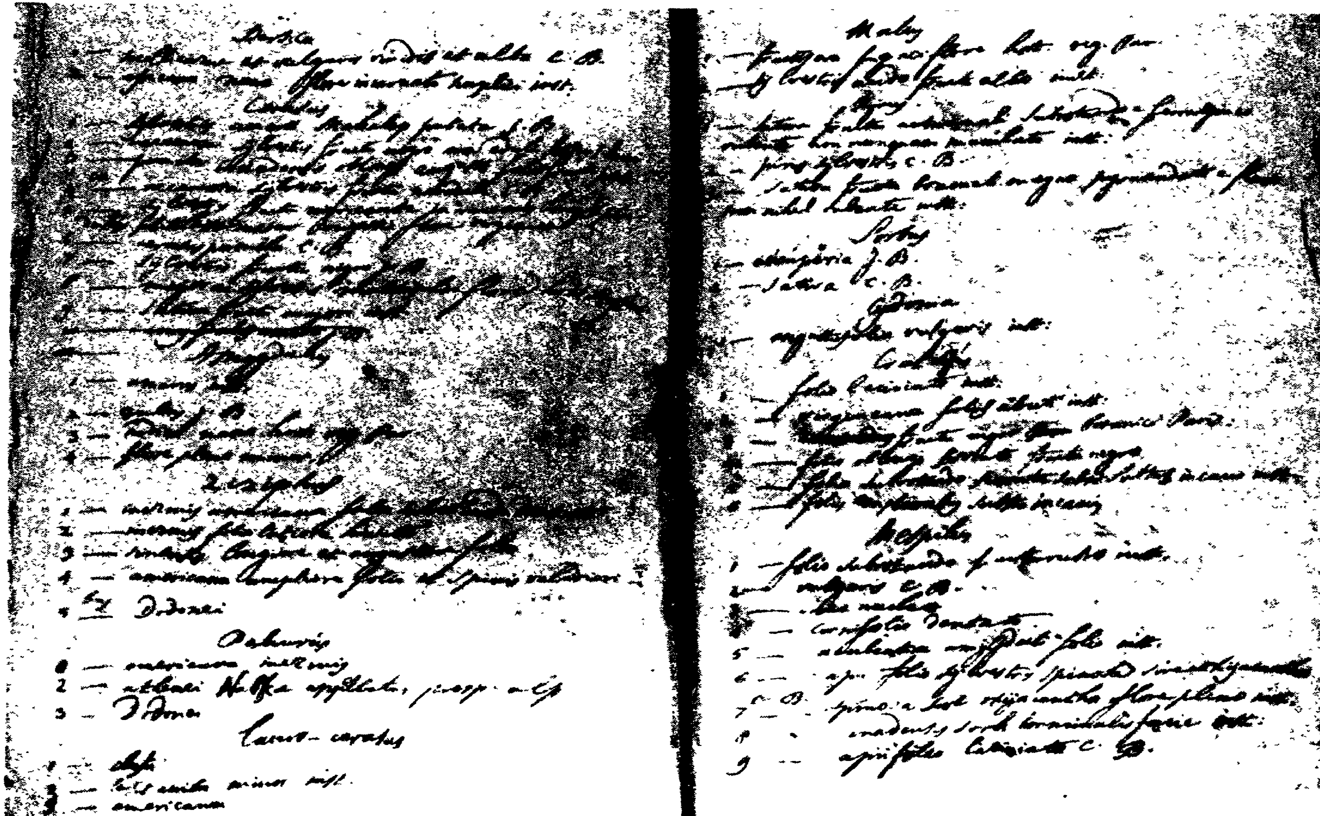
ON THE BEST MANNER OF JUDGING WHETHER THE PLANTS ARE READY TO YIELD THEIR DEPHLOGISTICATED AIR

As the light of the sun, and not the warmth, is the chief cause, if not the only one, which makes the plants yield their dephlogisticated air, it seems reasonable to think, that in a bright sun-shiny morning the plants will be earlier revived in their office than when the sun is hid by thick clouds. I found this difference to be very remarkable, so that in a dark cloudy morning I found the plants to begin their daily operation an hour or two later than in a clearer day. I even found that all the plants in the same garden did not awake, if I may so express myself, at the same time from their nocturnal stupor. Those plants, whose exposure was such as favoured the rays of the sun being cast early upon them, were revived earlier than those which were shaded by other plants, a wall, a house, &c. Nay, I even found that there was some difference in this respect between the leaves of the same tree; as I found those which were the first influenced by the light of the sun, the first ready to operate; when those of the opposite side of the tree, shaded by the first, were still in their state of stupor.

A ready way to know this time exactly is to put a leaf or two, from the plant you are to examine in this respect, in a glass full of fresh pump-water, and to observe, whether the bubbles appear upon them about as quick as they use to do in the full day time. If they do, you may be sure they are fit for the business.

But there is a readier way to know exactly this article of time, which I found by the water in the jar in which the green matter, discovered by Dr. Priestley, is already formed. The doctor found that this green matter yielded air bubbles only when placed in the sun; which observation served me as a good index, whether those plants which have experienced nearly the same influence of the sun as the green matter, were fit to yield dephlogisticated air. The more brisk you see these bubbles rise, the quicker your plants will give theirs.

But this manner of judging can only be of service in the morning; for in the middle of the day all leaves of plants, even those which were kept in a very dark place, revive so quickly, that they seem not to stand short in an observable way with those which were constantly in the open air.



TWO LEAVES FROM INGENHOUSZ' HERBARIUM (*plate 63*), WITH (*above*) TWO PAGES FROM A NOTE BOOK BELONGING TO THIS. (*Courtesy* Breda Municipal Archives and Museum for the History of Science at Leiden).

Section XXI

CONJECTURES WHY SOME WATERS, AS DISTILLED, BOILED, AND SOME OTHER WATERS, DO NOT PROMOTE, BUT IMPEDE THE OPERATION OF THE PLANTS IN YIELDING DEPHLOGISTICATED AIR

As I think I have proved clearly enough that the dephlogisticated air yielded by plants is air elaborated by a kind of vital motion, carried on in the substance of the leaves, and kept up by the influence of the light of the sun, it seems that no more is required to collect this air than to prevent its diffusing itself through the common mass of the atmosphere. Water seems the most appropriate body for such an intention, for it is not hurtful to plants. Many of them even thrive the best in it. The best quality required therefore in the water used for this purpose seems to be, to possess of itself air enough, so as not to imbibe it readily from the plants; and not so much as to be over-charged with it; for if the water is too much deprived of its own air, it must be more disposed to absorbe it from bodies plunged into it. And if water should be so much impregnated with any air, this air would readily rush into the substance of the leaves, and spoil by its bulk, or by its particular nature, the elaboration of the dephlogisticated air; the more so, as water, when found saturated with air, is found to possess this air in the form of fixed air, which differs too much from the nature of dephlogisticated air, or atmospheric air. Besides, water overcharged with air parts easily with it, which of consequence will of itself settle in the form of bubbles upon the leaves, and thus disturb their whole operation. We know that pump-water possesses of itself a great portion of air, which is generally thought to be for a part fixed air, to which it owes its agreeably pungent or brisk taste, which makes it palatable above all other waters. We know with more certainty, that boiled and distilled water are deprived of the greatest part of their air; and this is perhaps the reason, why they are not so palatable as common spring or pump-water. Therefore it seems to be not quite improbable, that water which has been boiled or distilled is very apt to absorbe itself the air which oozes out of the leaves, and that thus less air is gathered at the top of the bottle. This conjecture will perhaps find more ground from the following experiment. I placed some leaves of a vine in water, which I had, for this experiment, impregnated with fixed air: they were scarce under the surface of this water, but they were all covered with air bubbles; which seems to me to depend partly upon this water refusing to absorbe any air issuing from the leaves, because it was already overcharged with air itself. It is true that any other body, plunged in water impregnated with fixed air, will also become covered with air bubbles; but these bubbles do not appear so soon, or increase so rapidly, as those of the living leaves. So that it seems, that

the bubbles of the leaves increase faster because they are pushed out of the leaves by a vital motion in the leaf. It is also true, that leaves thus placed in water impregnated with fixed air, do not yield that fine dephlogisticated air which they yield when placed in common pump-water; which may be owing perhaps to the great abundance of fixed air penetrating the leaves, by being absorbed, and oozing out as it were, in a kind of tumultuary way, together with the air already contained in the leaves. Thus the air issuing out of the leaves may not have undergone that degree of elaboration required to change it into dephlogisticated air: for the least circumstance may disturb nature in this work; the shade of a building, or of another plant, may change this wonderful operation, so as to produce quite the reverse, and to obtain a poisonous air instead of dephlogisticated air: for the evaporation of bad air in the dark depends on the vital motion within the plant, which, being not influenced by the light of the sun, produces a contrary effect. Thus a plant growing in an absolute darkness is without green colour, and fruit without the influence of the light has no flavour.

Comments on Section XXI:— This discussion is such an antithesis to much that preceeded that it is difficult to understand, and the corresponding section in the French edition is even more of an enigma because he reasoned from wild analogies.

INGENHOUSZ had so clearly depicted the process of carbon dioxide absorption and oxygen elimination in preceding sections and emphasized the power of green plants to "purify" the air, that one is puzzled by his failure to grasp the fact that the same process occurred in the submerged leaves. His explanation of the behavior of leaves in boiled or distilled water and of leaves in water impregnated with carbon dioxide is, to say the least, naïve. He knew that pump water was the best for demonstrating the evolution of oxygen by leaves and believed that it contained carbon dioxide, "to which it owes its agreeably pungent or brisk taste, which makes it palatable above all other waters." It remained for SENEBIER to discover the true relation between the oxygen evolved, and the carbon dioxide and water absorbed by the leaf.

JOSEPH PRIESTLEY, in his pioneer work on the evolution of oxygen by plants, observed that the "green matter" appeared more readily in pump water than in rain or river water. Knowing that pump water contained appreciable amounts of "fixed air," he made experiments with water charged artificially with that gas. The results indicated that the evolution of dephlogisticated air was in some way dependent upon the presence of fixed air.

If PRIESTLEY had only recognized that the "green substance" was a growth of algae, he would have been able to put the cap stone on all his previous work. Although he did examine the green substance under the microscope, he was unable to determine its true nature and, unfortunately, failed to comprehend its function in separating oxygen from the water, concluding that the essential nature of the process was chemical, due to the action of light on the carbon dioxide in the water. Before he could correct his earlier views, the epoch-making work of INGENHOUSZ appeared. PRIESTLEY acknowledged in later editions of his works the errors into which he had been led.

INGENHOUSZ placed leaves of various plants under water and studied the gas which escaped when they were exposed to light. For him it was the only available method for collecting the eliminated gas, as he stated in Section 7. Under those conditions, the source of carbon dioxide would be the amount of that compound dissolved in the water plus varying amounts of carbonates and bicarbonates. He confirmed PRIESTLEY's discovery that oxygen is eliminated in the process. He was surprisingly oblivious to the fact that the green leaf, when illuminated, effects a synthesis of water and carbon dioxide to form a new compound. He thought that the water in his apparatus was ineffectual, except as a barrier to the common air. It remained for SENEBIER to discover, a few years later, that water, as well as carbon dioxide, entered into the reaction which we now call photosynthesis.

INGENHOUSZ attributed the source of the evolved oxygen either to air imprisoned in the leaf or to a metamorphosis of the water itself. He merely conjectured that the

air issuing from leaves submerged in water impregnated with "fixed air" may not have undergone that degree of elaboration required to change it into dephlogisticated air (oxygen), without having surmised that carbon dioxide in high concentrations may have a narcotic effect on leaves. Did he lack the equipment or the inclination to analyze the gases in the water? His observation that bubbles arose more quickly from leaves placed in the impregnated water indicates that his technique was good to a certain point, but other statements imply that the leaves were injured before the experiment was discontinued.

SENEBIER conducted simple experiments and determined the amounts of oxygen evolved by leaves in waters impregnated with carbon dioxide.

INGENHOUSZ indulged in various conjectures on the processes underlying the evolution of oxygen from submerged leaves. His nebulous ideas about chemistry are revealed in the way in which, here and elsewhere, he often confused phlogisticated air, or nitrogen, with fixed air, or carbon dioxide. One of the discrepancies which bothered him arose from the fact that some of the leaves which he submerged in water in the jars were more or less thickly beset with hairs, at least on the side which bore stomata, thereby holding a layer of air between water and the leaf. So long as the adherence of an intermediary film of air is shown by their silvery appearance, rapid gaseous exchange is possible, and hence such leaves may continue to assimilate fairly actively under water. In other experiments he used leaves which were not hairy and did not occlude a layer of air on their surfaces. It was more than a century later that BLACKMAN and SMITH (1910) determined the quantitative relations of assimilation in the aquatic plants *Elodea* and *Fontinalis* with satisfactory accuracy. Having found that the former plant assimilated about twice as much carbon dioxide as *Fontinalis* per unit of leaf mass, they concluded that the leaf of *Elodea* affords freer access of carbon dioxide to the chloroplasts than that of *Fontinalis*. The former has a well developed system of channels through which the "internal atmosphere" may pass, while the latter lacks this physiologico-anatomical feature.

SENEBIER set up series of tests in which carbonated water was compared with distilled and with boiled water, and with mixtures of the same. The amounts of gas evolved by the leaves were quantitatively determined. SENEBIER was clear in his arguments and concise in his statements that the leaves used the carbon dioxide in the water to produce the gas evolved. Where no carbon dioxide was present, the leaves evolved no gas. Where leaves produced gas, the impregnated water lost its carbon dioxide. "The amounts of gas evolved by the leaves in impregnated water when exposed to the light was so abundant, surpassing in volume that of the leaves which produced it, that we are obliged to admit that this gas furnished by the leaf is a gas which it has received from the water and is, up to a certain point, proportional to the amount of fixed air (carbon dioxide) contained in the water" (SENEBIER, 1788, Expériences, Second Mém., chap. III).

It is only in recent times that RUBEN, *et al.*, showed (1941) that the oxygen liberated during photosynthesis does indeed come from the water molecule.

Stimulated by SENEBIER's reports, INGENHOUSZ conducted new experiments, while working in Vienna, which showed him that the product depends upon the amount of fixed air added to the water. On this point INGENHOUSZ said, "When I saw this circumstance more clearly, I doubled my attention and began to believe that the water utilized by SENEBIER was far from being saturated with fixed air, as he seems to have believed, but probably was only lightly charged with fixed air."

It is one of the paradoxes of INGENHOUSZ' writings that he continued to believe that the oxygen evolved by submerged leaves arose from the air already present in the leaf.

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Section XXII

SOME REMARKS ON THE GREEN MATTER WHICH SETTLES AT THE BOTTOM AND SIDES OF THE JARS IN WHICH WATER IS LEFT STANDING

This green matter, which seems to be of the vegetable kind, was first found by the Rev. Dr. PRIESTLEY to yield very pure dephlogisticated air: but it ceases at last to yield more air if the water of the jar is not renewed, which ought therefore to be done now and then.

It is wonderful that this matter seems to be never exhausted of yielding dephlogisticated air, though it has no free communication with the common atmosphere, from which the most part of other plants seem to derive their stock of air. Does this vegetable matter imbibe the air from the water, and change it into dephlogisticated air? This does not seem to me probable, for I could not obtain from water, even by boiling, so much air as the water in which this substance was produced yielded by itself. I should rather incline to believe, that that wonderful power of nature, of changing one substance into another, and of promoting perpetually that transmutation of substances, which we may observe every where, is carried on in this green vegetable matter in a more ample and conspicuous way. The water itself, or some substance in the water, is, as I think, changed into this vegetation, and undergoes, by the influence of the sun shining upon it, in this very substance or kind of plants, such a *metamorphosis* as to become what we call now dephlogisticated air. This real transmutation, though wonderful to the eye of a philosopher, yet is no more extraordinary than the change of grass and other vegetables into fat within the body of a graminivorous animal, and the production of oil from the watery juice of an olive tree. More examples are to be seen of such wonderful transmutations of sublunary beings in the article upon the mutability of air.

On purpose to obtain in a short time a great deal of dephlogisticated air from this green matter, I gathered a good deal of it from the sides of a stone trough placed near a spring upon the high road, and always kept full of water for the horses. I put a good deal of this substance in a jar holding a gallon of pump-water, and inverted it in an earthen pan. In a week's time I found about 1½ pint of very fine dephlogisticated air collected in the jar, which surpassed in purity the air obtained in another jar from the green matter generated by itself. See experiment 100.

Cf. discussion of Count Rumford's work in Comments on Section IV, p. 332.

Section XXIII

IN PLANTING TREES FOR RENDERING THE AIR WHOLESOMER, IT SEEMS NOT TO BE QUITE INDIFFERENT WHAT KIND OF TREES ARE MADE USE OF

After what is already said on the subject, there will be no doubt left, that vegetables have a remarkable share in cleansing and purifying our atmosphere. But as it seems to follow from my experiments, that some trees yield by the day a purer dephlogisticated air than others, and that some seem to be less disposed to infect common air by night, it can scarce be considered as a matter entirely indifferent what kind of trees ought to be planted, if the salubrity of the air was the chief object of such a plantation. I made some experiments for this purpose, of which a few are placed in the second part of this book. But I am far from thinking myself entitled to decide any thing upon this head; the more, because all trees co-operate to the same end, and because the economical advantage arising from the preference of one sort of tree above another may be thought to over-balance the small advantage to be derived from its superiority in rendering the air purer. I must content myself with the discovery of the fact, and leave the rest to others, who, by farther and more decisive experiments, may have a better right to decide something upon this head than I can as yet pretend to.

Section XXIV

THE LARGEST AND THE MORE PERFECT LEAVES YIELD MORE AND PURER DEPHLOGISTICATED AIR, THAN THOSE WHICH ARE NOT YET FULL GROWN

Nothing seems to me a more convincing proof that the elaboration of dephlogisticated air is an effect of a kind of vital motion in the texture of the leaves, than that young leaves, not yet grown to their natural size, yield their air-bubbles slower and less in bulk, and that the air yielded by full-grown leaves surpasses in purity that which is obtained from leaves not yet come to perfection.

It is an amusing sight to observe in a jar full of pump-water the extremity of a branch of a vine, which contains leaves of different ages, from the maturest to those which only begin to unfold themselves. The air-bubbles make first their appearance upon the old leaves, then upon those that follow, and last of all on the new-born ones. The same proportion takes place also in the size of the bubbles; the largest or oldest leaves having always the largest bubbles, and therefore yielding far the greatest quantity of dephlogisticated air.

As it seems to be almost a constant rule, that the leaves which yield the greatest quantity of air, yield also the purest; the same rule also takes place in the old and new leaves. The young leaves seem not to have their organization compleated for the office to which they are destined, and therefore they are not yet able to elaborate so much nor so good dephlogisticated air as the old ones. The experiments 122 and 123 seem to be decisive in this respect.

Section XXV

THOUGH THE DIMINUTION OF THE BULK OF NITROUS AIR IS BELIEVED TO BE AN UNQUESTIONABLE TEST OF THE GOODNESS OF ANY AIR, YET, IT MUST BE ALLOWED, THAT IN SOME KINDS OF AIRS THIS TEST MAY FAIL

After having tried a great variety of airs myself, and after having seen many more tried by Abbé FONTANA, I no longer made the least doubt, but the discovery of Dr. PRIESTLEY in judging of the exact degree of goodness of any air was without any exception. But, as I was resolved to abstain as much as possible from all analogical conclusions, without they were supported by direct experiments, I tried every air I could find, not only by the nitrous test, but also by the flame of a candle, without, however, harbouring any mistrust in the already adopted manner of examining the degree of goodness of them.

I had already been convinced, that inflammable air was made explosive in a few hours when exposed in the sun with any plant, though I sometimes found it, by the nitrous test, so much corrected as to approach near to the goodness of common air. This gave me some suspicion, that this inflammable air might be susceptible of a still more remarkable correction or purification, at least in appearance, without losing its explosive quality.

On purpose to discover the whole, I left some inflammable air upon *persicaria,* and some upon wallnut leaves, during forty-eight hours, keeping the jars continually in the open air.

I tried first the air of the jar in which the wallnut leaves were, in the manner familiar to Dr. PRIESTLEY and in that of A. FONTANA; and repeated each trial twice with the same result. I found the air by both these methods to exhibit all the appearance of air, superior in quality to common air; as may be seen in experiments 110, 111, 113, 114, and 115; and yet I found this very air to explode with such a loud report, even in a cylindrical jar, that my servant, who kept the glass in his hands, thought it was absolutely broken. This event gave me no small concern for a method of trying the goodness of airs, which I had already considered as infallible. However, I had still some hopes left that I had committed some blunder in this experiment; and very luckily I had still at hand the jar which contained the *persicaria urens* with the inflammable air; but I was sorry to find that my suspicion was but too well grounded; for this air gave, by two different trials, the following result: one measure of it with one of nitrous air occupied 0.95; and with two measures of nitrous air 1.92; by A. FONTANA's method it gave 1.90, 1.96, 2.95; and thus it did appear by these trials to surpass far in goodness the common air; and yet it exploded at the flame of a candle with an uncommon loud report. See experiments 110 and 111.

There remained still one experiment to be tried with this air, *viz.*, to put a living animal in it. I was sorry to have spent the most part of this air, so as not to have enough of it left for this trial. However, I was resolved to push the experiment farther, and to let the inflammable air stand a longer while upon the plants, before it was to be employed for the different trials, and principally before an animal should be put in it. Some entire plants of *persicaria urens* were put in a gallon jar full of water, and as much strong inflammable air was let up as to fill above one third of the jar. I left it in the garden during six days, when I found, to my surprize, that it was very far from being corrected; for one measure of it, with one of nitrous air, occupied 1.80; it gave the following result by Abbé FONTANA's method, 2.58, 3.58: a chicken, near three weeks old, died in it in the space of one minute.

This result, so different from the former, greatly puzzled me, and restored my hope that the nitrous test was without exception, and that I must have committed some error in the former experiment.

I was, however, far from giving up entirely my suspicion of the failure of the nitrous test. I resolved to repeat the experiment again, with all possible attention; I had still half a pint left of the inflammable air, which had been during six days upon the *persicaria urens* without being much mended. See exp. 112. I put a fresh plant of mustard in a jar filled with water, and let up this inflammable air in the jar, so that the plant was in contact with the air. I placed it in the garden on a Saturday at twelve o'clock. I tried this air the next day between one and two in the afternoon, and found it by the nitrous test so much mended, that it appeared better than common air, and yet it exploded with a loud report by the approach of a candle. I replaced the jar again in the garden, and put the same air again to the nitrous test on the Monday following, when it appeared to be far superior to the atmospheric air, for one measure of it, with one of nitrous air, occupied 0.96; and yet it exploded as strongly as before. I replaced it again in the garden during four hours more, when it appeared to be still farther improved by the nitrous test, without losing, however, in the least, its explosive nature. See experiment 115.

I had also on the same Saturday put some plants of *persicaria urens* with their roots in a jar full of water, and let up two pints of a strong inflammable air. I found this air on Sunday, after the jar had been 24 hours in the garden, so much corrected, that it approached to the goodness of common air by the nitrous test, though it exploded with a loud report. I replaced the jar again in the garden, and again examined the air on Monday between one and two o'clock, when it appeared, by the nitrous test, about as good as common air; and yet it had not lost its explosive quality. After this, I replaced the jar as before, in the garden, and put the same air again to the test between four and five in the afternoon of the same day, when it appeared to be better than common air, without having lost its explosive force. There remained now nothing more to be done, than to try the effect of this air upon a living animal. I placed a lively chicken, three weeks old, in a jar filled with this air: it grew sick directly, and was in six minutes near dying, when I took it out quite motionless. It remained in the open air during several minutes in a dying condition, after which it gradually recovered.

I was now thoroughly convinced, that the nitrous test failed entirely in shewing the degree of salubrity of this air; for it appeared by this method to be nearly dephlogisticated air, and yet it was still a true poisonous air.*

I was indeed very sorry to find this failure in a method so well adapted for the exploration of atmospheric air. But I am very far from thinking that this exception diminishes in any way the real value of the important discovery, that *Nitrous air diminishes respirable air in the proportion to its salubrity.* For this test holds good in atmospheric air, which is the chief object of our experiments.

Comment on Section XXV:— Figures 2 to 6 on page 321 show the eudiometer and its parts: figures 7 to 9 show the glass measure and its mounting.

* Does this air owe its explosive nature to the dephlogisticated air oozing out of the plant? But this very air becomes also explosive, though it stands with a plant during the night, when the plants yield but a very small quantity of bad air. So that it rather appears to be changed by the plant in a kind of simple explosive air, or a true *fulminating air,* the only yet discovered, as far as I know.

Section XXVI

AIR IS ONE OF THE MOST CHANGEABLE SUBSTANCES IN NATURE, AND APPEARING UNDER VERY DIFFERENT FORMS AND QUALITIES FROM A VARIETY OF CAUSES

The air of our atmosphere is seldom during a whole day of the same quality. Its degree of wholesomeness is perhaps not less subject to variations than its weight and its degree of heat and cold. The barometer indicates the first, and the thermometer the other. But those two instruments seem to have no relation to the more or less purity of the atmosphere, or the more or less fitness of the air for the use of respiration.

The invention of an *Eudiometer,* or of an instrument or contrivance, by which the degree of purity of the common air, or its fitness for respiration, or rather its wholesomeness, can be investigated just as well as its weight, and its degree of heat and cold, is perhaps one of the most extraordinary inventions which ever was made.

We owe this important discovery to the Rev. Dr. PRIESTLEY. He found that nitrous air has the singular property of diminishing, or of being diminished by, common air in proportion to its goodness; or that the bulk of the two airs joined together contracts itself in a so much the narrower space, as the common air is better, purer, or more fit for respiration. It will soon appear to what a considerable degree of accuracy the Abbé FONTANA has brought this truly great discovery.

We have now in our hands the means of judging, not only of the degree of goodness of the common air upon the spot, but we may with as much ease also judge of the quality of the air of any country, by sending the air of it in close bottles. But as the air upon the same spot undergoes itself continual changes, we can but very seldom expect an accurate agreement of two experiments, unless made at the same time, or unless a quantity of the same air be shut up in a bottle sufficient for different experiments.

Until accurate instruments fit for such purposes are generally known, and employed with all the attention required, we shall not be able to judge of that degree of goodness which the air possesses for the most part of the year in a country, and thus to determine the advantages which would arrive to our constitution, in spending our lives in one country rather than in another, on purpose to preserve a good state of health, to cure particular diseases which require a pure air, or to protract our existence in this world in particular bodily dispositions. We must as yet content ourselves with the amusement of the experiment.

The continual changes which I observed in the atmosphere daily, by trying its constitution, convinced me of the too precipitate judgment of some philosophers, who, though furnished with but indifferent instruments,

have begun already to assert the degree of goodness of certain places, by one or two observations made in the time they passed through such a place. But I must leave the discussion of this matter to my respectable friend Abbé FONTANA, who, in my opinion, has cast a great light upon this important subject; and intends soon to publish his observations on this head. I will add only some further reflections upon the changeableness of air, its Proteus-like and metaphorical nature.

Since the experiments of the Rev. Dr. HALES, we know that air enters the composition of bodies, and even serves as a kind of cement for the stronger cohesion of the constituent particles of a solid body. By this it seems that air may become itself a solid body, as it constitutes such a considerable part of some particular bodies, such as are, for instance, vegetables, calcarious stone, nitre, &c. That a fluid body may become a solid, is nothing extraordinary; we see that water becomes as solid as a stone, and remains so, in a place sufficiently cold. There are perhaps in the world no substances which are by their nature *fluid:* for all substances yet found may be, by different operations, principally by a sufficient degree of heat, rendered fluid; and all fluids may be changed into solid bodies by applying to them a sufficient degree of cold. Mercury itself was rendered as malleable as any other metal, by Professor BROWN at St. Petersburg, by a very great degree of cold.

Since that kind of air is known, which goes now under the name of fixed air, and which VAN HELMONT, called *Gas Sylvestre,* it has been imagined by many, that different vegetables contain almost nothing but fixed air, because they saw that as soon as they began to ferment they emitted really fixed air. But if from this we conclude that this very same vegetable contained this fixed air, as such, concealed in its substance, and existing there, as it were in a concentrated or compressed state, almost as common air is in a condensing engine before the fermentation began, we may possibly make an erroneous conclusion; for it may be that this vegetable did not contain more fixed air as such than inflammable air; but that a part of the substance of the vegetable has undergone such a change by the action of the fermentation as to become what is now called fixed air, but what it was very far from being before the fermentation. That this may be the case I was induced to suspect by the following experiments: I squeezed the air out of different vegetables, keeping them under water, such as malva, potatoe-plant, hyoscyamus, apples, &c. I expected to find the most part of this air fixed air: but I was much disappointed; for this air was not diminishable by shaking it in water. By examining it in another way, I found that the flame of a wax-taper would grow dim in it, and that it was only somewhat inferior in quality to common air; for one measure of this air drawn from an apple, with one of nitrous air, occupied 1.24; and that expressed from the leaves of hyoscyamus occupied 1.25. The air expressed from malva and potatoe-plants appeared to be somewhat better. This air is undoubtedly the very air of the vegetable unaltered. I placed all those vegetables separately near the fire in water, and by examining the air disengaged from them I found it to be of a much worse quality than that which I obtained by squeezing; and by trying the air extracted from them by actual ebullition, I found it to be poisonous, and to extinguish flame. The air from an apple obtained by boiling was so

bad, that one measure of it with one of nitrous air occupied 1.71. Now these very plants, placed in the same water in the sun-shine, yield very fine dephlogisticated air, and by fermentation they yield fixed air. Is it therefore not probable, that the very air contained in the plant in its natural state was really an air approaching in quality to common air; and that the heat of the ebullition had changed this very air into phlogisticated air, in the same way as the act of fermentation changes it into fixed air, the light of the sun into fine dephlogisticated air, the digestion in the stomach and the intestines of an animal (a great deal of the air contained in the intestines, and all that from which we ease ourselves by the rectum, is pure inflammable air) and actual fire applied to it into inflammable air, and the obscurity of the night into another kind of truly poisonous air? Could it be said with any degree of probability, that one and the same vegetable contains these six kinds of air, so different in their nature, and even contrary to one another? Is it not more reasonable to say that vegetables contain an air, or by whatever name you will please to call it, which by undergoing different operations changes into different sorts of air?

Whoever therefore says, that such or such substance contains such or such air, because he extracts such air from it by the action of fire, by fermentation, or by any other means, may speak erroneously.

Nitrous acid, or spirit of nitre, yields nothing but nitrous air when it is poured upon mercury, copper, iron, &c.; but, when it is mixed with iron filings in a very diluted state, it gives, by the assistance of a moderate degree of heat, a mixture of different airs, partly fixed, partly common air, and partly phlogisticated air, (which experiment I saw at Abbé FONTANA's). When this very acid is joined to some earthy substance, or to a vegetable alkaline salt (with which it constitutes nitre), it yields by the action of the fire nothing but pure dephlogisticated air, in such abundance, that the quantity of it is equal to about eight hundred times the bulk of the nitre, as Abbé FONTANA found.

Such-like transmutations which air seems to undergo are every where obvious in nature. All bodies upon our earth, or almost all, undergo continually some alterations, and at last deviate entirely from what they were before. The plant which affords us the most wholesome food is perhaps the next to another which draws out of the same spot of ground a poisonous juice. The food by which a viper lives changes within his body into a substance which has nothing deleterious in itself, but in one place of its body a most virulent poison is elaborated from it. The same juice which the root of a tree pumps from the earth is changed into various fruits, very different in taste and qualities, if different sort of fruits are grafted upon it. An animal body becomes a manure for plants by corruption; it changes thus in the substance of a vegetable; this, being burnt, changes into ashes; which, by the action of the same fire, and by the addition of some sand and some calx of lead, changes into fine transparent glass. Thus what is now a part of our body may become in a short time a part of a pot or bottle.

The three mineral acids themselves may possibly be but one, and the same acid modified by some particular addition, which time may discover, to separate and thus to change marine acid into nitrous acid, &c. Some eminent chymists have already asserted this as their opinion. More or less

phlogiston in one acid than in another may make the one quite different in nature from the other. Common air impregnated with phlogiston makes a poisonous air; and common air, deprived of it, makes dephlogisticated air; in the one an animal dies in a little time; in the other it lives four or five times longer than in common air. Vitriolic acid extracts from iron its phlogiston, and allows it to impregnate the air disengaged in the act of solution. Nitrous acid disengages also the phlogiston from the iron, but does not allow it to pass in the air disengaged from it, so as to make it inflammable. It seems to keep this phlogiston to itself; for it is, after the solution, no more to be found in the dissolved iron, when precipitated in the form of ochre; but the same spirit of nitre, when dissolving iron in a very diluted state, leaves the most part of the phlogiston with the metal, and rises in the form of partly fixed air, common air, and what is called phlogisticated air, as was said above; and by this method iron may be reduced to the most impalpable powder, all obedient to the magnet, which is a method of making *Aethiops Martialis* of great importance for medical uses, and was discovered by an apothecary of Paris. Vitriolic acid extracts from calcareous earth, fixed air, and from some kinds of sparrs an air of a wonderful quality, corroding glass itself, which seems to be almost an incorruptible substance, and reducing it into dust by its contact only; and this air, so active upon glass, is by the first approach of water immediately reduced again into the form of the stone out of which it was extracted.

Considering all what is said before, I incline much to the opinion, that the various kinds of air extracted from the different bodies owe, for a great part, their specific nature to the transmutation which they undergo in the operation by which they are obtained; and that they cannot, at least not all, be said to exist in the body in a contracted state with more propriety than that glass exists actually in our body, because, by the action of fire, our body may be changed in a constituent part of that substance; and that fat exists in grass and other vegetables, because in the organs of an animal feeding upon these herbs they are partly changed into fat. Thus, when we feed upon vegetables, we do not in reality take in fixed air, existing as such in the substance of that food, and only let loose or extricated in our bowels; but it is more probable, that such food, undergoing in our stomach and intestines a kind of fermentation, yields really fixed air, not extricated, but generated by the act of fermentation.

As we have seen now, that common air is far from being an unalterable fluid, only to be changed by the addition of something, or by becoming deprived of something extraneous to its own original simple nature; we can no more be surprized to find, that the constitution of the atmosphere remains seldom a whole day the same, and that the degree of salubrity is continually changing. Indeed, in the course of three months, which I spent in my solitary retirement, I scarce found the degree of salubrity of the common air just the same during two days.

Those who are not yet acquainted with the accurateness of Abbé FONTANA's new *Eudiometer,* will be much inclined to believe, that the appearance of such continual variations is more owing to the imperfection of the method of exploring the air, than to the real changes happening in our element: and, indeed, I was much of that opinion, till Abbé FONTANA

convinced me of my error; for, by keeping a bottle full of air taken from the atmosphere at the same time, the constitution of it is explored and accurately registered; and examining some time afterwards this very air, closely shut up in a bottle, you find the result of the trial to correspond exactly with the result of that which was made at the time when the air was taken from the atmosphere, and by no means conform with the result of the trial instituted with the common air of the day, unless it should happen that the constitution of the atmosphere was just the same at both times. I take this to be a demonstrative proof of the excellence of this method, as well as of the erroneous judgement which any body might form of the accurate degree of goodness of the air of any given place, by examining it once or twice with nitrous air, principally if the observator is not in possession of an accurate instrument for making such an observation, or if he has not observed to the greatest nicety all the manoeuvres in the time of making the experiment.

It would be a difficult task to discover as yet the true causes of that continual fluctuation in the degree of salubrity of the air in the same place. But it seems to me not improbable, that this inconstancy is to be attributed in general to the natural changeableness of the air itself, by which it undergoes continual alterations from a variety of causes, of which a great number are perhaps not to be traced by human sagacity; and, indeed, if the air of a vegetable is from the nature of common air, or air approaching it, changed into true poisonous air, by applying only heat to it, as I have said already, and that some more or less light to which a plant is exposed changes its natural air into the most salubrious or the most poisonous air, may it not be suspected, with some degree of reason, that a great variety of causes, which have been till now overlooked, and which vary themselves continually, may bring on a very material alteration in our atmosphere, such as, for instance, heat and cold, dryness and moisture, light and obscurity, which I have already demonstrated to affect the operation of vegetables upon the air, winds blowing from different quarters, and conveying airs of different qualities, from distant countries, and many other operations of nature, unnoticed as yet?

Water itself, one of the simplest and the most unalterable substances known, seems to be changeable into dephlogisticated air, or at least to contain some things which may be transformed into this air by the influence of the day-light; for the green vegetable substance, which serves as a kind of laboratory, in which this salubrious air is produced, is formed from the water itself. Abbé FONTANA made a great many experiments tending to examine the air extracted from different waters by heat. I was present at these experiments in the summer of 1777, being then at Paris. He extracted from water of the Seine, and of the aqueduct of *Arcueil,* an air better than common air, which was a step towards the discovery of still better air from simple water, by some other way not yet hit upon. These interesting experiments are printed in the *Journal de Physique de l'Abbé Rosier,* May 1779.

Section XXVII

ON THE NATURE OF THE AIR OOZING OUT OF OUR SKIN

As our bodies perspire continually a watery liquid, either in an invisible way by what is called insensible perspiration, or by way of sweat, so a quantity of air seems to issue continually from the pores of our skin. This is easily to be observed in a cold or warm bath, in which we may clearly see whole bubbles of this air rising upon the skin, and at last rise to the top of the water. By plunging the hand and arm even in cold water, we may immediately observe a large number of those bubbles every where: and they are the more apparent when the skin is thoroughly dry before the part is plunged into the water; and much more so when it is plunged precipitately into it.

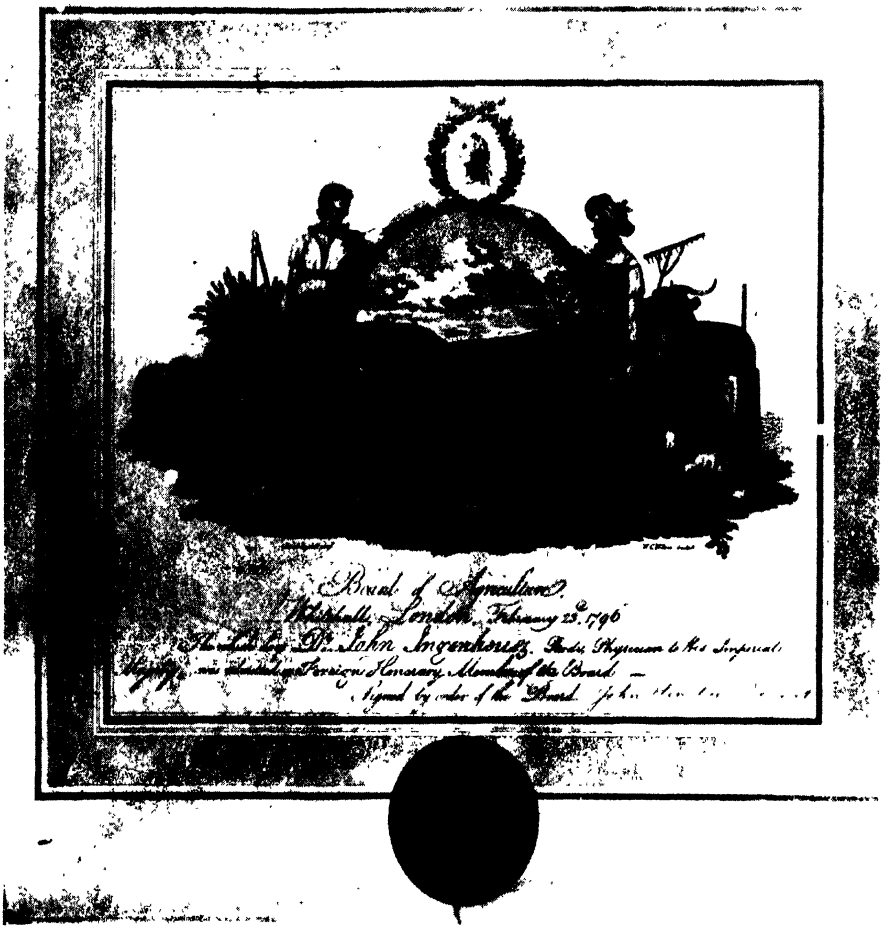
It is however to be observed, that all the air contained in those bubbles, which appear upon the skin, when a part of our body thoroughly dry is on a sudden plunged under the surface of the water, is not such as really issues out of the pores; for, as our skin is always covered with some unctuous matter which seems to repel water, the sudden immersion does not allow the water to *chace* before it has all the air sticking, as it were, to the skin, but a good deal of it is left upon it, and forms partly these large bubbles. This seems to be the more probable, as particularly those places to which these bubbles adhere are found quite dry, if observed attentively, when the part is withdrawn out of the water. But these very bubbles are in all probability also partly owing to air oozing out of the skin; for, if they were nothing but atmospheric air, they would not increase in size in cold water, but become smaller by condensation: now they increase even to a very large size in the coldest water, and at last detach themselves from the skin. A warm bath is not very proper to obtain the air oozing out of our skin. Water having been warmed has lost a good deal of the air naturally contained in it, and thus is very apt to absorb the air oozing out of the skin. The best water for this purpose is pump-water fresh drawn.

If we keep our arm, or any other part of the body, under water, and rub off all the air bubbles sticking to the skin, we shall see in a little while a great many small ones succeed. But the easiest way to convince one's self of the continual oozing out of air from our skin, is to rub the skin with the edge of an inverted glass full of water, and long enough to keep a good part of it above the surface of the water in the time the brim of it is sliding under water over the skin. In this way one may see an immense number of very minute bubbles rise continually to the top of the water in the inverted glass, and gather in larger bubbles at the top. By this method I collected, in a little time, from my arms, a measure of this air, which seemed to be partly fixed air, as it was somewhat absorbed by the water; at least, I thought to find the mass of it always less than it was before. This air put to the nitrous test was found far from being good

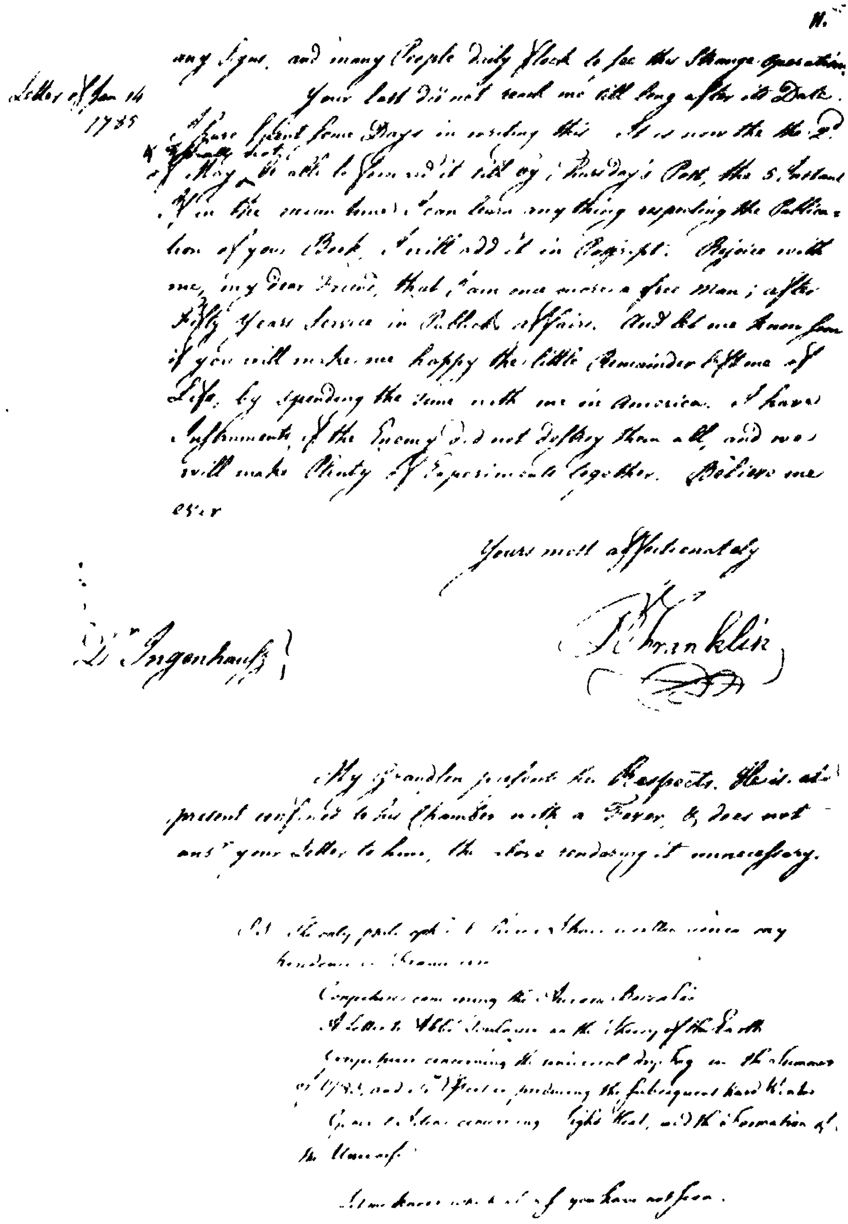
respirable air; for one measure of it, with one of nitrous air occupied, 1.46.

I took a quantity of air in like manner from the arms of a healthy person, 19 years old, and found one measure of it and one of nitrous air to occupy 1.84; which convinced me that the air evaporating from the skin of young people is not purer than that emitted from the skin of people more advanced in years; and that if there should be any advantage for old people to sleep in the same bed with young ones, as some imagine, it cannot likely be owing to their perspiring a better and wholesomer air from their skin. It is a very erroneous opinion, and even tending to do material mischief, that the air of a room, in which a great number of young people have been shut up, as in schools, is become very wholesome for old people to breathe in. I have seen school-masters so strongly prejudiced with this notion, that they even would not allow the windows of the school to be opened. for fear that the young air, as they called it, of the school-boys should escape; thinking that breathing this infectious and truly noxious evaporation would prolong their own life.

As I found that the bubbles appearing upon the skin, when a part of our body is plunged under water, are so much the larger as the part is put the more precipitately in the fluid, I could scarce doubt but the air gathered from these large bubbles must be for a great part atmospheric air, which could not so quickly detach itself from the skin, by the suddenness of the immersion; and I expected, therefore, that this air would give by the nitrous test a better appearance than that gathered from the small bubbles scraped from the skin by the edge of a glass. I gathered from another young and healthy person the air of the large bubbles found upon the skin, by plunging the arm suddenly under the water. And I found it approaching more to the nature of common air, though a candle could not have burned in it, nor an animal breathe in it without anxiety; for one measure of it with one of nitrous air occupied 1.40.



DIPLOMA PRESENTED TO INGENHOUSZ BY THE BOARD OF AGRICULTURE (LONDON) IN 1796 (*Courtesy* Breda Municipal Archives).


N.

any Sign; and many People daily flock to see the Strange Operation.

Letter of Jan 14 1785 — Your last did not reach me till long after its Date. I have spent some Days in writing this. It is now the [illegible] of May [illegible] to send it till my Thursday's Post, the 5. Instant. If in the mean time I can learn any thing respecting the Publication of your Book, I will add it in Postscript. Rejoice with me, my dear Friend, that I am once more a free Man; after Fifty Years Service in Publick Affairs. And let me know from you if you will make me happy the little Remainder left me of Life, by spending the time with me in America. I have Instruments if the Enemy did not destroy them all, and we will make Plenty of Experiments together. Believe me ever

Yours most affectionately

B Franklin

Dr Ingenhausz

My Grandson presents his Respects. He is at present confined to his Chamber with a Fever, & does not answer your Letter to him, the above rendering it unnecessary.

P.S. The only [illegible]

Conjectures concerning the Aurora Borealis

A Letter to Abbé Soulavie on the Theory of the Earth

Conjectures concerning the universal Fog in the Summer of 1783, and its Effects in producing the subsequent hard Winter

Queries & Conjectures concerning Light, Heat, and the Formation of the Universe.

Some of these I suppose you have not seen.

Page from a letter from Benjamin Franklin to Jan Ingenhousz (1700), *cf.* pp. 390 and 391. (*Courtesy* Library of Congress, Washington, D. C.).

CONCLUSION

I hope the indulgent reader will excuse in me a small degree of vanity, in flattering myself with having discovered a law of nature hitherto entirely unknown, and hid till now behind the screen of that awful darkness which overcasts our earth during the time it withdraws its surface from the direct influence of that all-reviving luminary, the sun.

I flatter myself also to have put beyond all doubt, that the vegetables have a remarkable share in keeping up the salubrity of our atmosphere, by imbibing those septic, noxious, and phlogistic particles, which were communicated to it by the breathing of so many animals which inhabit the surface of the earth, and by many other causes; as well as by pouring down a most beneficial shower of purified or dephlogisticated air, which, diffusing itself through the mass of common air, counteracts the general causes, tending to contaminate our atmosphere, and to render it unfit for the use of respiration. I was lucky enough to discover that the *vegetation* itself of the plants has nothing to do with the cleansing our atmosphere; but that this great work is operated by the influence of the sun's light, exciting and keeping up the vital and intestine motion of these numberless fans, which the most part of plants display at once, just at the time when the general tendency to corruption is increased by the increase of heat.

Though we are too much accustomed to look upon the most obvious operations of nature with a kind of unconcern and indifference, such as, for instance, the vegetation of plants; yet we cannot look with so much indifference upon the final causes of those every where obvious scenes when we discover them; for they do not so much affect the organs of our sight and other external senses, as they do our understanding, our reason, our judgment; by which only we are superior to all other living animals. The consideration of final causes gives us to understand that this great *universe* is not the offspring of chance, not coeval with the beginning of time, or of an eternal origin; but that it has been made by an Omnipotent Being, who, by giving it existence, has, at the same time, endowed it with most wonderful qualities and powers, continually in action, and tending with an astonishing harmony to one general end, the preservation of the whole.

An upright mind, averse to that manner of living which induces many to wish, rather than really to believe, that this world is not superintended by an intelligent Being, takes delight in finding out those deep designs, which, by their obvious tendency to promote the preservation of the whole, inspire him with that awful reverence we owe to the Supreme Cause of every thing, and fill him with that consoling expectation, that the only being upon earth capable of true reason, and of tracing the existence of a God in his wonderful works, and of contemplating him in adoration, may expect not to be entirely annihilated after his body is returned into dust, out of which it took its origin.

But to come back from this digression to the purpose, let us consider how much the real facts drawn from nature itself are concordant with the theory deduced from my experiments. If the leaves of vegetables have really a considerable share in cleansing the atmosphere, it must happen, that the time, when our common air is the purest, is the summer and the winter; for in the summer the plants are in their greatest vigour; and in the middle of the winter the causes of general corruption are the most checked by the cold. Now this is just what happens. As soon as in the advanced autumn the leaves begin to wither and to fall, and to contribute even somewhat themselves to contaminate the air by their corruption, the degree of purity of the atmosphere is really less than it was during the time of the summer; and this atmosphere does not return to its former good quality till the winter is set in, and till the remaining tendency to corruption is checked by the increase of cold. In the spring, when the sun begins to promote somewhat the general tendency to corruption, without having yet influence enough upon the vegetables to make them display their leaves, the common air begins to be less fit for respiration, till it returns again to its

former purity as soon as the leaves are produced. And this is what Abbé FONTANA found to be a constant fact.

If I had more leisure, I should be inclined to expatiate in a wide and open field of reflections, which present themselves to my mind, and to draw all the consequences which seem to flow, as from a fountain-head, from the already mentioned observations.

Is it not probable, that those who labour under consumptive and asthmatic complaints, and who find the greatest relief, and sometimes a perfect cure, by retiring to mild climates, where vegetation is lively, and begins sooner in the spring, should go to such places where the constitution of the air is found by experience to be during the whole year the best? But these places will not be known till some accurate method of examining the goodness of common air be in general use.

Is it not somewhat probable, that it is unsafe for the health of people to sleep in rooms having windows towards a small open place crouded with the branches of a large tree, so hidden from the influence of the sun as to receive but seldom its rays? I remember to have heard people say, that it was unwholesome to sit under a wallnut-tree, and that they found themselves affected by its shade. But I looked upon such an apprehension as one of those popular or vulgar errors which are propagated from father to son. I should now be inclined to think, that an apprehension of some mischief might not be entirely ill-grounded, when such a tree stands, as is often the case in a narrow yard confined by the surrounding buildings.

It is a general belief in the West Indies, founded upon constant experience, that the mangeneel-tree *Hippomane Mancinella* (Linn. Spec. Plant. 1431) throws out very hurtful exhalations, so as to endanger those people, who, ignorant of the nature of this tree, venture to lay down under it.

The plant called *Lobelia Longiflora*, growing also in the West Indies, spreads such deleterious exhalations from it, that a considerable oppression is felt upon the breast in approaching, at several feet distance, this plant, placed in the corner of a hot-house or room. (See the description of this plant in *Jacquini Hortus Botanicus Vindobonensis*).

The plant called *Dictamnus Albus*, or *Fraxinella*, which is by no means rare throughout almost all Europe, when in flower spreads round about inflammable air, which, by the approach of a candle by night, flashes as other inflammable air does. We know that an animal breathing in this kind of air loses its life: so that if a man was to sleep with his head in the middle of the branches of this plant, he might run a risk of being killed by it.

May we not ascribe the unwholesomeness of the air of that immense plain in which *Rome* stands to the want of trees and other vegetables? That very plain was, in ancient times, reputed to be a very wholesome country, when it was well cultivated and inhabited. And in our days, being not far from a real desert, it is so notorious for being unwholesome, that the people of the country think it highly dangerous to pass a single night in it, even in the middle of the summer; whereas in Tuscany, which is peopled and cultivated to the utmost, one may sleep the whole summer in the open air without fearing more injury from it than from the air within the house. The Pontine Lake, *Lacus Pontinus*, in the dominions of the pope, in which formerly, when cultivated, were numbers of inhabitants, supplying Rome with the best productions of the earth, is at present a most dismal desert, spreading round about it unwholesome and deadly exhalations, so that scarce any living animal can breathe this air without soon losing its health, and finding its destruction.

The want of proper cultivation contributes, perhaps, not a little towards rendering the immense plains of Hungary less wholesome than they would otherwise be. The country round about Vienna is perhaps likewise in want of a sufficient number of trees.

POSTSCRIPT

As I went on with my experiments during the whole time this book was printing, I continued to discover more and more the secret operations of nature in regard to cleansing our atmosphere. I have carefully registered in my notes the result of the experiments, which I may possibly communicate to the publick in a second volume, together with some more deductions which I may draw from my remarks.

Though I am obliged abruptly to stop my further researches, I cannot dismiss the reader without acquainting him, that, as soon as the warm weather began to cease, and the autumnal colds to set in (the thermometer of Fahrenheit being under 50 in the shade, which had been in the time of the other experiments in general between 70 and 83), the leaves, fruits, and roots had lost a good deal of their mischievous influence upon the circumambient air in the night, and by day in shaded places, though they had lost nothing of their salubrious power in yielding by day dephlogisticated air; but that the flowers seemed to have lost very little or nothing of their malignant effluvia by which they contaminate the surrounding air; and that water standing by itself, or with plants in it, loses by the sun-shine, or rather by the warmth communicated to it in the sun, the faculty of promoting, or rather of not obstructing, the plants yielding dephlogisticated air; but that it recovers almost to an equal degree its former faculty, by the coldness of the night. Water, in which I found ice in the morning, and which the day before obstructed the leaves in yielding a tolerable quantity of bubbles, was so much recovered, when it was heated by the sun, that fresh leaves put in it yielded air-bubbles very briskly, when the thermometer plunged in it was at 37.

From what has been said in the nineteenth Section, as well as from other experiments, I am more and more induced to believe that our atmospheric air is a substance of a very changeable nature, and that it is, in common with a great many other substances, equally liable to become worse, or of undergoing a kind of corruption by the increase of heat; and that this tendency to corruption is checked by the vital operation of the plants in the summer, and by the cold in the winter. By this observation, we may perhaps be induced to believe, that those countries which are very hot in the summer, and are little or not cultivated, as is a great part of Hungary and the country round about Rome, are not only exposed to have their air contaminated by the breathing of animals in it, and by the corruption of many other substances, but also by the corruption which the air itself is liable to undergo during the heat of the season; and which mischief can chiefly be remedied by making a sufficient quantity of vegetables grow in them, principally trees. Draining the marshes, and preventing inundations by keeping the rivers within their bounds by dykes, and by cutting canals to let out the water, will greatly assist the operation of vegetables, which would be insufficient to cleanse the atmosphere of low countries, without this great cause of corruption, owing to marshes, being removed.

APPENDICES

EXPÉRIENCES
SUR
LES VÉGÉTAUX,

SPÉCIALEMENT ſur la Propriété qu'ils poſſèdent à un haut degré, ſoit d'améliorer l'Air quand ils ſont au ſoleil, ſoit de le corrompre la nuit, ou lorſqu'ils ſont à l'ombre ;

Auxquelles on a joint une Méthode nouvelle de juger du degré de ſalubrité de l'Atmoſphère :

Par JEAN INGEN-HOUSZ, Conſeiller Aulique, & Médecin du corps de Sa Majeſté l'Empereur & Roi, Membre de la Société royale de Londres ; de la Société Philoſophique Américaine de Philadelphie, *&c. &c.*

TOME SECOND.

A PARIS,
Chez THÉOPHILE BARROIS le jeune, Libraire, quai des Auguſtins, n°. 18.

M. DCC. LXXXIX.
Avec Approbation, & Privilège du Roi.

TITLE PAGE OF THE SECOND VOLUME OF THE FIRST FRENCH EDITION OF INGENHOUSZ' 'EXPERIMENTS' (1789).

EXPERIENCES
SUR
LES VÉGÉTAUX

PREMIÈRE PARTIE

OBSERVATIONS SUR LA NATURE DES PLANTES

SECTION PREMIÈRE

Quelques Remarques Générales sur la Nature des Feuilles des Plantes, & sur leur Usage:— Dès que le soleil commence, au printemps, à répandre sur la surface de la terre de la chaleur à un certain degré, la plupart des végetaux, surtout les arbres, offrent en peu de jours le spectacle le plus beau, la décoration la plus frappante. Ce même degré de chaleur qui ranime la végétation, est, par sa nature, une cause générale de corruption. Les plantes contractées & engourdies pendant le froid de l'hiver, ne présentent d'autre surface que celle du tronc & des branches, comme si elles vouloient se cacher & se soustraire à l'air autant qu'elles le peuvent; elles augmentent en peu de jours leur surface, peut-être plus de mille fois, en poussant ces espèces d'éventails sans nombre, qui nous sont connus sous le nom de feuilles. Un changement aussi notable arrivant presque subitement, & donnant une nouvelle apparence à la surface de la terre, semble indiquer un dessein particulier, dont l'importance doit être proportionnée à la grandeur de la scène.

Ceux qui se sont occupés à observer les ouvrages de la nature, n'ont pas manqué d'admirer cet appareil majestueux dont elle se revêt au printemps, en poussant les feuilles; & quelques-uns, éblouis de ce nouveau spectacle, ont cru que l'Auteur de ce monde avoit pour principal but, en produisant les feuilles, de flatter nos yeux par une décoration ravissante, & de nous garantir de l'ardeur du soleil par la fraîcheur de l'ombre. D'autres, plus philosophes, ont pensé que les feuilles servent à pomper l'humidité de l'air, de la rosée, des pluies, à favoriser la végétation & la fructification, par conséquent, la propagation de l'espèce; & ainsi ils ont presque entièrement réduit l'usage des feuilles à l'utilité de la plante dont elles constituent une partie si considérable.

Il est hors de doute que les feuilles contribuent beaucoup à la vigueur de la végétation; car, lorsqu'on en dépouille l'arbre, on le met en danger de périr. En arrosant les feuilles & le tronc d'un jeune arbre, on augmente considérablement sa croissance; ce qui prouve que les feuilles lui rendent un service essential en absorbant l'humidité. Les feuilles favorisent la fructification & la propagation de l'espèce; car, si on en dérobe une grande partie à l'arbre, il languit, le fruit n'acquiert pas le goût qui lui est propre: &, en dépouillant l'arbre entièrement, le fruit tombe avant sa maturité. Quoique l'importance des feuilles soit assez apparente par les usages exposés ci-dessus, on n'aura cependant pas de peine à croire qu'elles soient encore destinées à des objets qui n'ont aucun rapport avec la fructification, dés qu'on considéra que la fructification est déjà très-avancée dans un grand nombre de plantes, avant que les feuilles paroissent; que dans d'autres, elles poussant des mois entiers avant les fleurs; & que beaucoup de plantes conservent leurs feuilles dans la plus grande vigueur, & même continuent d'en pousser de nouvelles, après que la fructification est entiérement achevée.

Il est vrai que les utilités multipliées des feuilles n'ont rien de bien extraordinaire; car il n'y a peut-être aucune production de la nature bornée à un seul usage: nous voyons que les fruits n'ont pas été faits seulement pour contenir la semence & propager l'espèce de la plante, mais qu'ils sont destinés en même temps à servir d'aliment aux animaux, de remèdes à leurs maladies, &c. Les semences de quantité de plantes n'ont certainement pas pour seul but de propager leur espèce; car il y a des plantes qui

en produisent tant, que, si elles n'étoient pas recueillies, à peine un centième trouveroit-il de la place pour végéter. Nos facultés sont trop bornées pour découvrir toutes les causes finales de cette immensité d'êtres qui nous environment, & dont nous ne connoissons ni la nature, ni l'utilité. Chaque découverte que nous faisons dans les opérations que la nature avoit tenues jusqu'alors sous le voile, nous montre de plus en plus la sagesse suprême de son Auteur. On doit présumer qu'il est entré dans son plan de former les différens êtres qu'il contient, de manière à leur faire remplir toutes les vues dont ils sont susceptibles, & à se prêter des secours mutuels.

Mon dessein n'étant pas d'entrer dans un grand détail sur ce qui regarde la constructions des feuilles, & les rapports qu'elles ont avec la végétation de la plante, mais de découvrir la relation que ces organes ont avec le règne animal, & les avantages que nous en tirons; j'abandonnerai le reste à ceux qui ont fait une étude particulière de cette branche d'histoire naturelle. On peut consulter sur ce sujet le superbe Ouvrage Anglois *in-folio* de NEHEMJAW GREW, *the Anatomy of plants,* publié à Londres en 1682, avec grand nombre de très-belles Planches; les Observations microscopiques contenues dans les ouvrages de M. LEUWENHOEK, de M. BAKER; les Observations & Expériences de THUMMINGIUS sur l'anatomie des feuilles dans le Journal de Leipsik, 1722, pag. 22; ce qu'on trouve dans l'utile ouvrage de M. VALMONT DE BOMARE, Dictionnaire d'Histoire naturelle, sur-tout l'article *Utilités des Feuilles, leur Examen au microscope,* &c. Extraits de la Physique des Arbres de DUHAMEL.

Le célèbre M. BONNET, de Genève, a publié un des plus savans ouvrages sur ce sujet; il a pour titre: *Recherches sur l'usage des Feuilles dans les plantes, & sur quelques autres sujets relatifs à l'Histoire de la Végétation,* par CHARLES BONNET, à Gottingen & Leiden, 1754. Cet ouvrage contient sur la nature, les propriétés & les usages de ces organes merveilleux, un grand nombre de recherches intéressantes, faites avec la plus grande attention, qui ont répandu beaucoup de lumière sur ce sujet.

Il a remarqué les bulles d'air qui couvrent les feuilles des plantes lorsqu'on les plonge sous l'eau; il dit, page 26, que ces bulles, dont la surface inférieure se couvre, sont de l'air que la feuille sépare de l'eau qu'elle a imbibée. Impatient de vérifier ce soupçon, il fit bouillir de l'eau pendant trois-quarts d'heure, afin de chasser l'air qu'elle contient; il y plongea une branche de vigne, & les bulles ne parurent pas, quoique le soleil fût ardent; ensuite il imprégna l'eau d'air, en soufflant dedans, & les bulles parurent & devinrent plus grandes. Il dit, page 28, qu'elles se montrent ordinairement lorsque le soleil commence à échauffer l'eau, & qu'elles disparoissent à l'approche de la nuit, à cause du froid. A la page 31, les ayant observées plus soigneusement, il dit qu'il a appris par l'expérience, que ces bulles sont produites par l'air adhérent aux teuilles sèches, logé dans leurs inégalités, & dilaté par la chaleur du soleil, & que ces bulles disparoissent à l'entrée de la nuit, l'air qui les formoit étant condensé par la fraîcheur; que pour cette même raison les bulles cessent de se former vers ce temps. A la page 33, il dit que ce ne sont pas seulement les feuilles plongées vivantes dans l'eau, qui s'y couvrent de bulles; qu'il en a aussi observé sur des feuilles mortes, & cueillies depuis plus d'un an; que ce fait achève de démontrer que les bulles qui s'élèvent sur les feuilles vertes, & qui végétent encore, ne sont pas l'effet de quelque mouvement vital. Je puis en fournir, dit-il, une autre preuve. Ayant retiré de l'eau des feuilles vertes très-chargées de bulles, ces bulles se sont crevées dans l'air, & la place qu'elles occupoient sur la feuille a été très-facile à reconnoître, parce qu'elle n'étoit point humectée; l'eau ne l'avoit pas encore touchée.

Ayant examiné avec toute l'attention dont je suis capable, la production de ces bulles, j'ai cru qu'elles étoient d'une importance beaucoup plus grande que M. BONNET ne l'imaginoit. Voici à peu près ce que j'ai observé par rapport à leur apparition.

La plupart des feuilles se couvrent de ces bulles, lorsqu'on les plonge sous une eau quelconque au soleil, ou en plein jour, dans un lieu ouvert & bien éclairé, mais infiniment plus dans l'eau de source fraîchement tirée; elles sortent plus lentement, & en moins grand nombre, sur les feuilles plongées dans l'eau de rivière; moins encore dans l'eau de pluie, & moins que dans toute autre dans l'eau stagnante des marais, l'eau bouillie ou distillée. Les feuilles couvertes d'eau bouillie ou distillée, étant exposées au soleil, ne se couvrent communément d'aucune bulle d'air, parce que ces eaux étant privées de tout air, absorbent celui que les feuilles exhalent au soleil, à mesure qu'il sort de leurs pores. Elles ne sont pas produites, parce que la chaleur du soleil raréfie l'air adhérent aux feuilles; car beaucoup produisent des bulles dans l'instant même qu'on les plonge dans l'eau la plus froide, quoiqu'elles soient, dans le

moment qu'on les sépare de l'arbre, & qu'on les plonge dans l'eau, échauffées par le soleil; elles ne poussent pas de bulles d'air après le coucher du soleil, ou du moins fort peu; mais celles qui étoient déja sorties ne disparoissent point, malgré le froid de la nuit.

Comme les feuilles, lors même qu'elles sont échauffées par la chaleur du soleil, rendent ces bulles d'air presque aussi-tôt qu'elles sont plongées sous l'eau, quoique celle-ci soit très-froide en comparaison du degré de chaleur qu'elles ont, il paroît très-clair que ces bulles ne sont pas dues à la raréfaction de quelque air adhérent aux feuilles, ni même d'un air qui existât déjà dans leur substance; car la fraîcheur de l'eau récemment tirée des entrailles de la terre, devoit plutôt resserrer leurs pores & condenser l'air qui pouvoit s'y trouver. D'un autre côté, l'apparition subite de ces bulles, & leur accroissement qui se fait par degré dans l'eau froide exposée à la clartè du jour, la cessation de cette émission d'air pendant la nuit, & dans l'ombre pendant le jour, dans la même eau, semblent indiquer que ces bulles ne doivent pas leur origine à l'air existant dans l'eau, & pompé par les feuilles, ni à la raréfaction de l'air déja adhérent aux feuilles, mais à quelque mouvement vital qui a lieu dans les feuilles exposées au grand jour, & qui cesse dès qu'elles se trouvent à l'ombre; & il semble que la sortie de cet air, sous la forme de bulles, n'est que la continuation des courans ou jets de ce même air, qui sortent des conduits excrétoires des feuilles pendant la grande clarté du jour, mais qui sont de la plus grande subtilité, &, dans l'état naturel des choses, parfaitement invisibles.

Nous ne faisons donc que surprendre la nature sur le fait, en plongeant les feuilles toutes vivantes sous l'eau, dans laquelle elles restent en vigueur, & par conséquent peuvent continuer une partie de l'opération à laquelle elles étoient occupées immédiatement auparavant. Je dis que les feuilles, dans ces circonstances, peuvent continuer *en partie* leur travail; car, quoiqu'elles puissent, dans l'eau, répandre leur air, comme hors de l'eau, elles ne peuvent cependant plus en absorber de nouveau de la masse de l'atmosphère, parce que l'eau qui les entoure intercepte leur communication avec l'atmosphère. Il est donc très-probable que si les feuilles rendent par leurs pores une quantité d'air si considérable, lors même qu'elles ne peuvent pas réparer cette perte par l'absorption d'un air nouveau, elles en donnent une quantité bien plus grande dans l'état naturel, où elles peuvent en absorber autant qu'elles en perdent (*a*).

Si nous examinons l'air qui forme ces bulles, nous serons bientôt convaincus qu'il est bien loin d'être de l'air commun; nous le trouverons d'une qualité beaucoup supérieure au meilleur air de l'atmosphère; il est véritablement *déphlogistiqué:* un animal y vit beaucoup plus long-temps que dans l'air commun le plus pur; il augmente considérablement le volume de la flamme d'une bougie, elle y acquiert un éclat qui éblouit les yeux; & une bougie éteinte y reprend la flamme, s'il lui reste la moindre particule du feu.

Ce fluide éthéré, que les feuilles répandent en grande abondance, comme une pluie bienfaisante, mais invisible, doit naturellement contribuer beaucoup à purifier l'atmosphère; il est peut-être une des principales causes qui préservent la race des animaux de la destruction, quand la chaleur augmente la corruption générale de tant de corps qui, par leurs exhalaisons nuisibles, infectent continuellement l'air, & le rendent moins capable de soutenir la vie. Lorsque le froid de l'hiver arrête cette tendance universelle vers la corruption, nous n'avons plus besoin de l'assistance des feuilles pour purifier notre atmosphère, qui n'est plus tant infectée. Les feuilles tombent, & l'arbre continuant à vivre sans elles, nous announce qu'elles avoient plus de rapport à notre conservation qu'à la sienne. Dans les climats chauds, où la source générale de la corruption, la chaleur, ne cesse pas d'exister, la verdure est perpétuelle.

Les feuilles, aussi-tôt qu'elles se sont développées, s'arrangent entre elles de la manière la plus convenable pour ne pas s'embarrasser les unes les autres, & elles exposent leur surface vernie, autant qu'il est possible, à l'influence directs du soleil, en cachant l'inférieure à ses rayons, comme si elles cherchoient plus sa lumière que sa chaleur; car le vernis de cette surface, exposé à ses rayons, doit, en les réfléchissant, modérer la chaleur.

On verra par la suite, qu'il est probable que la surface inférieure des feuilles a été destinée principalement à répandre l'air purifié; la supérieure, à absorber l'air

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• J'ai développé cette doctrine ultérieurement dans deux mémoires insérés dans le Journal de Physique, l'un au cahier du mois de Juin 1784, l'autre au cahier du mois de mai 1785.

atmosphérique, & à l'élaborer en air déphlogistiqué, en sequestrant le principe inflammable dont il est toujours souillé; & que cette opération se fait au moyen d'un mouvement intestin & vital, excité & entretenu par l'action de la lumière. Cette probabilité deviendra plus plausible, si nous considérons que, par un tel arrangement, l'air déphlogistiqué, sortant de la surface inférieure des feuilles, trouve moins d'obstacle à sa descente; que l'air déphlogistiqué est spécifiquement plus pesant que l'air atmosphérique, & que par conséquent il doit, par sa nature, être porté à descendre. Nous trouverons ce systême encore plus vraisemblable si nous y ajoutons que la plupart des airs nuisibles aux animaux, sont plus légers que l'air commun, &, par conséquent, doivent être disposée à monter; que pour cette raison, l'air méphitique que les feuilles des plantes exhalent pendant l'obscurité de la nuit (*a*), celui qui sort des eaux stagnantes & des substances dans l'état de corruption, &c. montent vers les régions élevées de l'atmosphère, & qu'ainsi nous sommes délivrés presque aussi-tôt qu'il est produit.

De tout ceci, nous pouvons recueillir de nouvelles lumières sur l'arrangement des différentes parties de ce monde, & sur la dépendence des êtres les uns des autres, sur les secours mutuels qu'ils sont destinés, par l'Auteur de la nature, à se prêter pour le maintien du tout. Nous verrons que les plantes, en séparant de l'air atmosphérique le principe inflammable, ou le phlogistique, rejettent le superflu en air déphlogistiqué, comme un fluide devenu nuisible à elles-mêmes mais alors très-salutaire aux animaux; & que les animaux, après avoir fait leur profit de cet air épuré, en le respirant, le rendent à leur tour aux plantes, chargé du phlogistique surabondant de leur corps, un des principaux alimens des végétaux.

En un mot, nous verrons que la nature a confié aux feuilles des plantes une fonction bien plus noble que celle qu'on leur avoit attribuée jusqu'à présent, & que nous n'avons aucune raison d'être de mauvaise humeur en voyant éclore presque par-tout cette foule d'orties, de chardons & autres végétaux, à qui notre ignorance a donné le nom injurieux de *mauvaises herbes.*

FRANKLIN to INGENHOUSZ[1]

London, March 18, 1774

Dear Friend: I am very sensible of your kindness in the concern you express on account of the late attack on my character before the Privy Council and in the papers. Be assured, my good friend, that I have done nothing unjustifiable, nothing but what is consistent with the man of honour and with my duty to my king and country, and this will soon be apparent to the public as it is now to all here who know me. I do not find that I have lost a single friend on the occasion. All have visited me repeatedly with affectionate assurances of their unaltered respect and affection, and many of distinction, with whom I had before but slight acquaintance. You know that in England there is every day, in almost every paper, some abuse on public persons of all parties, the king himself does not always escape, and the populace who are used to it love to have a good character cut up now and then for their entertainment. On this occasion it suited the purpose of the ministry to have me abused, as it often suits the purpose of their opposers to abuse them. And having myself been long engaged in public business, this treatment is not new to me. I am almost as much used to it as they are themselves, and perhaps can bear it better. I have indeed lost a little place that was in their power, but I can do very well without it. It will not be long before I publish my vindication, which some circumstances keep back at present.

Sir John Pringle continues well. His speech in giving the last medal, on the subject of the discoveries relating to the air, did him great honour. Dr. Priestley goes on rapidly with new and curious experiments on that subject. He is about printing a new 8*vo* book full of them . . .

B. Franklin

(*a*) Il sera parlé ailleurs plus amplement de la nature de cet air.

[1] *From* John Bigelow, "The Complete Works of Benjamin Franklin," Vol. X, p. 337.

FRANKLIN to INGENHOUSZ[1]

Passy, 21 June, 1782

Dear Sir: I am sorry that any misunderstanding should arise between you and Dr. .... The indiscretions of friends on both sides often occasion such misunderstandings. When they produce public altercations, the ignorant are diverted at the expense of the learned. I hope, therefore, that you will omit the polemic piece in your French edition, and take no public notice of the improper behavior of your friend, but go on with your excellent experiments, produce facts, improve science, and do good to mankind. Reputation will follow, and the little injustices of contemporary laborers will be forgotten; my example may encourage you, or else I should not mention it. You know that when my papers were first published, the Abbé NOLLET, then high in reputation, attacked them in a book of letters. An answer was expected from me, but I made none to that book, nor to any other. They are now all neglected, and the truth seems to be established. You can always employ your time better than in polemics.

M. LAVOISIER the other day showed an experiment at the Academy of Sciences, to the Count DU NORD, that is said to be curious. He kindled a hollow charcoal, and blew into it a stream of dephlogisticated air. In this focus, which is said to be the hottest fire human art has yet been able to produce, he melted platina in a few minutes.

Our American affairs wear a better aspect now than at any time heretofore. Our counsels are perfectly united; our people all armed and disciplined. Much and frequent service as militia has indeed made them soldiers. Our enemies are much diminished, and reduced to two or three garrisons. Our commerce and agriculture flourish. England at length sees the difficulty of conquering us, and no longer demands submission, but asks for peace. She would now think herself happy to obtain a federal union with us, and will endeavor it; but, perhaps, will be disappointed, as it is the interest of all Europe to prevent it. I last year requested of Congress to release me from this service, that I might spend the evening of life more agreeably in philosophic leisure; but I was refused. If I had succeeded, it was my intention to make the tour of Italy with my grandson, pass into Germany, and spend some time happily with you, whom I have always loved, ever since I knew you, with uninterrupted affection.

We have lost our common friend, the excellent PRINGLE. How many pleasing hours you and I have passed together in his company! I must soon follow him, being now in my seventy-seventh year; but you have yet a prospect of many years of usefulness still before you, which I hope you will fully enjoy; and I am persuaded you will ever kindly remember your truly affectionate friend,

B. FRANKLIN

[1] *From the* "Complete Works of Benjamin Franklin" by JOHN BIGELOW, Vol. VII, p. 473, 1888.

GENERAL INDEX

Contents of Chronica Botanica, Vol. 11 (1947/48)

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